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" Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster
vilior quia ex alienis libamus ut apes." Just. Lips. *Polit.* lib. i. cap. 1. Not.

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"Meditationis est perscrutari occulta; contemplationis est admirari perspicua . . . . . Admiratio generat quæstionem, quæstio investigationem, investigatio inventionem."—*Hugo de S. Victore.*

—"Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phœbus ferrugine condat,
Quid toties diros cogat flagrare cometas;
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu."

J. B. Pinelli ad Mazonium.

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NUMBER CCLI.—JUNE.

PLATE.

Illustrative of Prof. R. Bunsen's Paper on the Washing of Precipitates.

I. *On the Washing of Precipitates.* By R. Bunsen[*].

[With a Plate.]

A PRECIPITATE is washed either by filtration or by decantation : in the former case the portion of liquid not mechanically retained is allowed to drain from the precipitate ; in the latter it is separated by simply pouring it away, the foreign substances contained in the precipitate being then removed by the repeated addition of some washing-fluid, in each successive portion of which the precipitate is, as far as possible, uniformly suspended, this process being continued until the amount of impurity becomes so minute that its presence may be entirely disregarded.

Supposing v to represent the volume of the moist precipitate remaining at the bottom of the vessel after decantation, or upon the filtrate after filtration, V the volume of wash-water employed at each successive decantation, n the number of decantations, and $\frac{1}{a}$ the fraction expressing the proportion of the original amount of impurity still remaining in the precipitate after n decantations, then

$$\left(\frac{v}{v+V}\right)^n = \frac{1}{a}. \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Calling W the total volume of wash-water resulting from n de-

cantations, then

$$nV = W ; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

therefore

$$\left(1 + \frac{W}{nv}\right)^n = a,$$

or

$$W = nv \left(\sqrt[n]{a} - 1\right). \quad . \quad . \quad . \quad . \quad . \quad (3$$

If we differentiate W with respect to n and make the differential quotient equal to 0, then the minimum value of W becomes, when $n = \infty$,

$$W = v \text{ nat. log. } a. \quad . \quad . \quad . \quad . \quad . \quad (4)$$

Precipitates obtained in the course of chemical analysis may in all cases be assumed to be sufficiently washed when the impurity retained by them amounts to no more than the $\frac{1}{100000}$ part. Making therefore $a = 100000$ and $v = 1$, it results from equation (4) that the least quantity of fluid required in order to remove the impurity contained in a precipitate to the $\frac{1}{100000}$ part, amounts to eleven and a half times the volume occupied by the precipitate itself in the liquid in which it exists. It is evident, therefore, that the amount of water actually necessary to wash a precipitate the more nearly approaches this minimum the oftener we decant, and the smaller the quantity of washing-water we employ at each decantation.

Since some of the principal sources of error in analytical work consist in the incomplete or in the too protracted washing of precipitates, it becomes important to know how to ascertain the progress of the washing throughout the several stages of the process. By employing the same volume of water at each successive addition, and estimating its relation to that of the precipitate remaining at the bottom of the vessel or upon the filter, we can find from the following Table, calculated by means of the formula above given, the number of times it is necessary to decant in order to diminish the amount of impurity in the precipitate to the $\frac{1}{100000}$, $\frac{1}{50000}$, $\frac{1}{20000}$, or $\frac{1}{10000}$ part. Column I. shows the relation between the volume of the precipitate and that of the washing-water employed for each successive decantation, column II. the number of decantations required to diminish the amount of impurity to the necessary extent, and column III. the total volume of water obtained from the several decantations.

$\dfrac{1}{100000}$			$\dfrac{1}{50000}$			$\dfrac{1}{20000}$			$\dfrac{1}{10000}$		
I.	II.	III.	I.	II.	III.	I.	II.	III.	I.	II.	III.
$\dfrac{V}{v}$	$n.$	W.	$\dfrac{V}{v}$	$n.$	W.	$\dfrac{V}{v}$	$n.$	W.	$\dfrac{V}{v}$	$n.$	W.
0·5	28·4	14·2	0·5	26·7	13·3	0·5	24·4	12·2	0·5	22·7	11·4
1	16·6	16·6	1	15·6	15·6	1	14·3	14·3	1	13·3	13·3
2	10·5	21·0	2	9·8	19·7	2	9·0	18·0	2	8·4	16·8
3	8·3	24·9	3	7·8	23·4	3	7·1	21·4	3	6·6	19·9
4	7·1	28·6	4	6·7	26·9	4	6·1	24·6	4	5·7	22·9
5	6·4	32·1	5	6·0	30·2	5	5·5	27·6	5	5·1	25·7
6	5·9	35·5	6	5·6	33·4	6	5·1	30·5	6	4·7	28·4
7	5·5	38·8	7	5·2	36·4	7	4·8	33·3	7	4·4	31·0
8	5·2	42·0	8	4·9	39·4	8	4·5	36·1	8	4·2	33·5
9	5·0	45·0	9	4·7	42·3	9	4·3	38·7	9	4·0	36·0
10	4·8	48·0	10	4·5	45·1	10	4·1	41·3	10	3·8	38·4
11	4·6	51·0	11	4·4	47·9	11	4·0	43·8	11	3·7	40·8
12	4·5	53·9	12	4·2	50·6	12	3·9	46·3	12	3·6	43·1
13	4·4	56·4	13	4·1	53·3	13	3·8	48·8	13	3·5	45·1
14	4·2	59·4	14	4·0	55·8	14	3·7	51·1	14	3·4	47·5
15	4·2	62·3	15	3·9	58·5	15	3·6	53·6	15	3·3	49·8
16	4·1	65·0	16	3·8	61·1	16	3·5	56·0	16	3·3	53·0
17	4·0	67·8	17	3·7	63·6	17	3·4	58·3	17	3·2	54·2
18	3·9	70·4	18	3·7	66·1	18	3·4	60·5	18	3·1	56·3
19	3·8	74·3	19	3·6	68·6	19	3·3	62·8	19	3·1	58·4

When the washing-process is performed in a beaker, the relation between the volume of the precipitate and that of the liquid may be easily determined by holding a strip of paper along the side of the vessel and marking upon it the respective heights of the precipitate and supernatant liquid; then on folding the portion of paper lying between the two marks in such a manner that each fold corresponds to the height occupied by the precipitate, the number of folds will give the argument in column I. to find in column II. the number of decantations needed to wash to the required extent. If the washing be conducted as in the ordinary method of filtration, funnels possessing an angle of 60° must be invariably employed, and the capacities of the various-sized filters once for all determined by means of a burette. After the precipitate has been brought upon the filter and allowed to drain, it is mixed as thoroughly as possible with water from a graduated washing-flask. Call the amount of water thus necessary to fill the filter v, and the capacity of the empty filter $\mathfrak{V}$, then $\dfrac{v}{\mathfrak{V}-v}=\dfrac{V}{v}$ in column I.; that is, the argument needed to find in column II. the number of times it is necessary to refill the filter in order to wash the precipitate to the desired extent.

I by far prefer using this Table to employing the method generally followed of ascertaining the completion of the washing-process by evaporating a quantity of the filtrate on platinum-foil, since in the latter case it is only possible to obtain an infallible proof when we have to deal with a precipitate possessing an extremely high degree of insolubility; if the precipitate be soluble to any marked extent, the result is completely illusory.

In the process of filtration as hitherto conducted, the time required is so long and the quantity of wash-water needed so great that some simplification of this continually recurring operation is in the highest degree desirable. The following method, which depends, not upon the removal of the impurity by simple attenuation, but upon its displacement by forcing the wash-water through the precipitate, appears to me to combine all the requisite conditions and therefore to satisfy the need.

The rapidity with which a liquid filters depends, *cæteris paribus,* upon the difference which exists between the pressure upon its upper and lower surfaces. Supposing the filter to consist of a solid substance, the pores of which suffer no alteration by pressure or by any other influence, then the volume of liquid filtered in the unit of time is nearly proportional to the difference in pressure : this is clearly shown by the following experiments, made with pure water and a filter consisting of a thin plate of artificial pumice-stone. The thin plate of pumice was hermetically fastened into a funnel consisting of a graduated cylindrical glass vessel, the lower end of which was connected with a large thick flask by means of a tightly fitting caoutchouc cork. The pressure in the flask was then reduced by rarefying the air by means of a method to be described upon another occasion ; and for each difference of pressure p, measured by a mercury column, the number of seconds t was observed which a given quantity of water occupied in passing through the filter. The following are the results :—

I.

p. metre.	t.	pt.
0·179	91·7	16·4
0·190	81·0	15·4
0·282	52·9	14·9
0·472	33·0	15·6

In the ordinary process of filtration, p on the average amounts to no more than 0·004 to 0·008 metre. The advantage gained, therefore, is easily perceived when we can succeed by some simple practicable and easily attainable method in multiplying this difference in pressure one or two hundred times, or, say, to an entire atmosphere, without running any risk of breaking the

filter. The solution of this problem is very easy : an ordinary glass funnel has only to be so arranged that the filter can be completely adjusted to its sides even to the very apex of the cone. For this purpose a glass funnel is chosen possessing an angle of 60°, or as nearly 60° as possible, the walls of which must be completely free from inequalities of every description ; and into it is placed a second funnel made of exceedingly thin platinum-foil, and the sides of which possess exactly the same inclination as those of the glass funnel. An ordinary paper filter is then introduced into this compound funnel in the usual manner ; when carefully moistened and so adjusted that no air-bubbles are visible between it and the glass, this filter, when filled with a liquid, will support the pressure of an extra atmosphere without ever breaking.

The platinum funnel is easily made from thin platinum-foil in the following manner :—In the carefully chosen glass funnel is placed a *perfectly accurately fitting* filter made of writing-paper ; this is kept in position by dropping a little melted sealing-wax between its upper edge and the glass ; the paper is next saturated with oil and filled with liquid plaster of paris, and before the mixture solidifies a small wooden handle is placed in the centre. After an hour or so the plaster cone with the adhering paper filter can be withdrawn by means of the handle from the funnel, to which it accurately corresponds. The paper on the outside of the cone is again covered with oil, and the whole carefully inserted into liquid plaster of paris contained in a small crucible 4 or 5 centims. in height. After the mixture has solidified, the cone may be easily withdrawn ; the adhering paper filter is then detached, and any small pieces of paper still remaining removed by gently rubbing with the finger. In this manner a solid cone is obtained accurately fitting into a hollow cone, and of which the angle of inclination perfectly corresponds with that of the glass funnel.

Fig. 1 (Plate I.) represents the cones. By their help the small platinum funnel is made. A piece of platinum (fig. 2 shows the natural size) is cut from foil of such a thickness that one square centimetre weighs about 0·154 grm., and from the centre a a vertical incision is made by the scissors to the edge $c\,b\,d$. The small piece of foil is next rendered pliable by being heated to redness, and is placed upon the solid cone in such a manner that its centre a touches the apex of the latter ; the sides a,b,d are then closely pressed upon the plaster, and the remaining portion of the platinum wrapped as equally and as closely as possible around the cone. On again heating the foil to redness, pressing it once more upon the cone, and inserting the whole into the hollow cone and turning it round once or twice under a

gentle pressure, the proper shape is completed.   The platinum funnel, which should not allow of the transmission of light through its extreme point, even now possesses such stability that it may be immediately employed for any purpose.   If desired, it may be made still stronger by soldering down the overlapping portion in one spot only to the upper edge of the foil by means of a grain or two of gold and borax ; in general, however, this precaution is unnecessary.   If the shape has in any degree altered during this latter process, it is simply necessary to drop the platinum funnel into the hollow cone and then to insert the solid cone, when by one or two turns of the latter the proper form may be immediately restored.   The platinum funnel is placed in the bottom of the glass funnel, the dry paper filter then introduced in the ordinary manner, moistened, and freed from all adhering air-bubbles by pressure with the finger.   A filter so arranged and in perfect contact with the glass, when filled with a liquid will support the pressure of an entire atmosphere without the least danger of breaking ; and the interspace between the folds of the platinum-foil is perfectly sufficient to allow of the passage of a continuous stream of water.

In order to be able to produce the additional pressure of an atmosphere, the filtered liquid is received in a strong glass flask instead of in beakers*. This flask is closed by means of a doubly perforated caoutchouc cork, through one of the holes of which the neck of the glass funnel is passed to a depth of *from* 5 *to* 8 *centimetres* (fig. 3) ; through the other is fitted a narrow tube open at both ends, the lower end of which is brought *exactly to the level of the lower surface of the cork,* to the other is adapted the caoutchouc tube connected with the apparatus (fig. 4) destined to produce the requisite difference in pressure : this apparatus will be described immediately.   The flasks are placed in a metallic or porcelain vessel (fig. 3), in the conical contraction of which several strips of cloth are fastened.   This method of supporting the flask has the advantage that, in one and the same vessel, flasks varying in size from 0·5 to 2·5 litres stand equally well, and that, by simply laying a cloth over the mouth of the vessel, the consequences of an explosion (which through inexperience or carelessness is possible) are rendered harmless.

It is impossible to employ any of the air-pumps at present in use to create the difference in pressure, since the filtrate not unfrequently contains chlorine, sulphurous acid, hydric sulphide, and other substances which would act injuriously upon the metallic portions of these instruments.   I therefore employ a *water*

* These flasks must be somewhat thicker than those ordinarily used, in order to prevent the possibility of their giving way under the atmospheric pressure.

air-pump constructed on the principle of Sprengel's mercury-pump, and which appears to me preferable to all other forms of air-pump for chemical purposes, since it effects a rarefaction to within 6 or 12 millimetres pressure of mercury.

Fig 4 shows the arrangement of this pump. On opening the pinchcock a, water flows from the tube l into the enlarged glass vessel b, and thence down the leaden pipe c. This pipe has a diameter of about 8 millims., and extends downwards to a depth of 30 or 40 feet, and ends in a sewer or other arrangement serving to convey the water away. The lower end of the tube d possesses a narrow opening; it is hermetically sealed into the wider tube b, and reaches nearly to the bottom of the latter. A manometer is attached to the upper continuation of this tube d by means of a side tube at d^1; at d^2 is attached a strong thick caoutchouc tube possessing an internal diameter of 5 millims. and an external diameter of 12 millims.; this leads to the flask which is to be rendered vacuous, and is connected with it by means of the short narrowed tube k. Between the air-pump and the flask is placed the small thick glass vessel f, in which, when one washes with hot water, the steam which may be carried over is condensed. All the caoutchouc joinings are made with very thick tubing, the internal diameter of which amounts to about 5 millims., the external diameter to about 17 millims. The entire arrangement is screwed down upon a board fastened to the wall, in such a manner that each separate piece of the apparatus is held by a single fastening only, in order to prevent the tubes being strained and broken by the possible warping of the board. On releasing the pinchcock a, water flows from the conduit l down the tube c to a depth of more than 30 feet, carrying with it the air which it sucks through the small opening of the tube d in the form of a continuous stream of bubbles. No advantage is gained by increasing the rapidity of the flow, since the friction exerted by the water upon the sides of the leaden pipe acts directly as a counter pressure, and a comparatively small increase in the rapidity of the flow is accompanied by a great increase in the amount of this friction. Accordingly at g is a second pinchcock, by which the stream can be once for all so regulated that, on completely opening the cock a, the friction, on account of the diminished rate of flow, is rendered sufficiently small to allow of the maximum degree of rarefaction. Such an apparatus, when properly regulated once for all by means of the cock g, exhausts in a comparatively short time the largest vessels to within a pressure of mercury equal to the tension of aqueous vapour at the temperature possessed by the stream[*]. The tension exerted

[*] The time required to obtain the above degree of exhaustion in a flask of from 1 to 3 litres capacity ranges from six to ten minutes; the quantity of water necessary amounts to about 40 or 50 litres.

by the water-stream in my laboratory, in which six of these pumps are used, amounts to about 7 millims. in winter and 10 millims. in summer.   The filtration is made in the following manner:— The flask standing in the metallic or porcelain vessel (fig. 3) is connected by means of the slightly drawn-out tube *k* with the caoutchouc tube *h* attached to the pump, the cock *a* having been previously opened and the properly fitted moistened filter filled with the liquid to be filtered.   As usual, the clear supernatant fluid is first poured upon the filter ; in a moment or two the fil- trate runs through in a continuous stream, often so rapidly that one must hasten to keep up the supply of liquid, since it is ad- visable to maintain the filter as full as possible.   After the pre- cipitate has been entirely transferred, the filtrate passes through drop by drop, and the manometer not unfrequently now shows a pressure of an extra atmosphere.   The filter may be filled (in fact this is to be recommended) with the precipitate to within a millimetre of its edge, since the precipitate, in consequence of the high pressure to which it is subjected, becomes squeezed into a thin layer broken up by innumerable fissures.   As soon as the liquid has passed through and the first traces of this breaking up become evident, the precipitate will be found to have been so firmly pressed upon the paper, that on cautiously pouring water over it it remains completely undisturbed.   The washing is effected by carefully pouring water down the side of the funnel to within a centimetre *above* the rim of the filter : the washing- flask for this purpose is not applicable ; the water must be poured from an open vessel.   After the filter has in this manner been replenished four times with water and allowed to drain for a few minutes, it will be found to be already so far dried, in conse- quence of the high pressure to which it has been subjected, that without any further desiccation it may be withdrawn, together with the precipitate, from the funnel, and immediately ignited, with the precautions to be presently given, in the crucible.

If the porosity of a paper filter containing a precipitate were as unalterable as that of a pumice-stone filter, the experiments above described would show that the times required for filtration, according to the old method on the one hand, and the new one on the other, would be inversely proportional to the difference in pressure in each case ; that is, by using the pump under the full pressure of about 740 millims., the time needed to wash a pre- cipitate, occupying by the old process an hour, would at the utmost not amount to more than 30 seconds.   In using these pumice filters (about which I will speak presently) to drain crystals from adhering mother-liquors, or, say, to wash crystals of chromic acid by means of concentrated sulphuric acid and fuming nitric acid, the time occupied in the filtration is scarcely

longer than that needed to pour a liquid slowly from one vessel to another. In filtering by means of paper, the precipitate gradually closes up the pores of the filter, and accordingly such an extraordinary acceleration as this can no longer be expected. But the following examples will show the saving of time and labour the method effects, even under all unfavourable conditions. For these experiments I have purposely chosen the hydrated chromium sesquioxide, since it is one of the most difficult of precipitates to wash thoroughly. A solution of chromium chloride was prepared by acting with fuming hydrochloric acid upon potassium dichromate; and by means of a measuring-vessel, which allowed the amount of chromium to be estimated to within 0·0001 grm., successive portions of the liquid were withdrawn, and the chromium oxide contained in them precipitated with the usual precautions by ammonia. The volume of liquid, the quantity of ammonia employed, the time occupied in boiling and in permitting the precipitate to settle, the angle of inclination possessed by the funnel, and the size of the filter were the same in all the experiments. All the precipitates were washed with hot water, and, after burning the filter, ignited over the blowpipe for a few minutes; in weighing, the platinum crucible was tared by one of about equal weight, and the position of equilibrium of the beam determined by vibrations.

I first attempted to filter one of the precipitates in the ordinary way. $\frac{V}{v}$ amounted to 2; and consequently, from the Table, 8·4 fresh additions of water were required in order to wash the precipitate to the $\frac{1}{10000}$ part. The times required were as follows:—

In transferring the precipitate from the beaker and allowing it to drain . .			40′
For the first addition of water to run through.			48
,, second ,, ,,			70
,, third ,, ,,			80
Total length of time			238

At this point the experiment was discontinued, as the filtrate became turbid. A second experiment failed from the same cause.

Accordingly I attempted to wash the precipitate by decantation. The volume of the precipitate amounted to about 30 cub. centims.: the quantity of water required to fill the beaker was seven times the volume of the precipitate; hence $\frac{V}{v}$ was 7, and the

requisite number of decantations to reduce the amount of impurity to the $\frac{1}{50000}$ part was 5·2. The times observed were as follows :—

II.

For the first decantation to run through the filter.					15
,,	second	,,	,,	,,	12
,,	third	,,	,,	,,	18
,,	fourth	,,	,,	,,	15
,,	fifth	,,	,,	,,	18
In transferring the precipitate to the filter				. .	30

Time required in washing 108

Weight of the precipitate . . . 0·2458 grm.
Volume of wash-water nV . . . 1050 cub. centims.

III.

Experiment repeated.   Number of decantations 7.   Other circumstances the same as in the foregoing determination.

Time required in washing . . . 140′
Weight of the precipitate . . . 0·2452 grm.
Volume of wash-water . . . . 1200 cub. centims.

IV.

After ten decantations.

Time required in washing   . . 180′
Weight of the precipitate   . . 0·2443 grm.
Volume of wash-water   . . . 1750 cub. centims.

By filtration with the platinum cone and the pump the following results were obtained :—

V.

In transferring the precipitate to the filter (17 cub. centims. water)					6
For the first addition of water (25 cub. cent.) to run through.					2
,,	second	,,	,,	,,	3
,,	third	,,	,,	,,	2
,,	fourth	,,	,,	,,	2
,,	fifth	,,	,,	,,	2
In draining the precipitate					2

Time required 19

Weight of precipitate . . . . 0·2435 grm.
Volume of wash-water . . . . 142 cub. centims.
Pressure in manometer   . . . 0·576 metre.

VI.

In transferring the precipitate and allowing the water (18 cub. centims.) to run through . . . . } 8'

For the first addition of water (25 cub. cent.) to run through. 4
,,   second   ,,   ,,   ,,   ,,   5
,,   third   ,,   ,,   ,,   ,,   5
,,   fourth   ,,   ,,   ,,   ,,   5
In draining the precipitate 1

Time required 28

Weight of precipitate . . . . 0·2434 grm.
Volume of wash-water . . . . 118 cub. centims.
Pressure . . . . 0·600 metre.

VII.

In transferring the precipitate and allowing the water (20 cub. centims.) to run through . - . . . . . } 4'

For the first addition of water (25 cub. cent.) to run through. 3
,,   second   ,,   ,,   ,,   ,,   3
,,   third   ,,   ,,   ,,   ,,   3
In draining the precipitate 3

Time required 16

Weight of precipitate . . . . 0·2432 grm.
Volume of wash-water . . . . 95 cub. centims.
Pressure . . . . 0·584 metre.

VIII.

In transferring with 25 cub. centims. of water. 8'
For the first addition of 25 cub. centims. to run through . . . . } 5
For the second addition of 25 cub. centims. to run through . . . . } 5
In draining the precipitate 3

Time required 21

Weight of precipitate . . . 0·2435 grm.
Volume of wash-water . . 72 cub. centims.
Pressure . . . . 0·593 metre.

IX.

In transferring with 15 cub. centims. of water and allowing it to run through . . . } 7'
For a single addition to run through . . . 3
In draining the precipitate 2

Time required 12

Weight of precipitate . . 0·2439 grm.
Volume of wash-water . . 41 cub. centims.
Pressure . . . . 0·572 metre.

X.

In transferring the precipitate with 13 cub. centims. of water . . . . } $\frac{1}{5}$

For a single addition of water (26 cub. centims.) to run through . . . . . } 8

In draining the precipitate 1

    Time required 14

Weight of precipitate . . . 0·2439 grm.
Volume of wash-water . . 39 cub. centims.
Pressure . . . . 0·530 metre.

In washing, by means of decantation, in the ordinary manner, the amounts of chromium sesquioxide found were as follows :—

grm.
II. 0·2458, after 5 decantations, washed to the $\frac{1}{50000}$ part.
III. 0·2452 ,, 7   ,,   ,, $\frac{1}{200000}$ part.
IV. 0·2443 ,, 10   ,,   ,, $\frac{1}{100000000}$ part.
 0·2451 mean.

By the use of the pump :—

grm.
V. 0·2435, after 5 additions of water.
VI. 0·2434 ,, 4 ,,   ,,
VII. 0·2432 ,, 3 ,,   ,,
VIII. 0·2435 ,, 2 ,,   ,,
IX. 0·2439 ,, 1 addition of water.
X. 0·2439 ,, 1 ,,   ,,
 0·2436 mean.

Hence the probable amount of chromium sesquioxide contained in the solution, according to the experiments with the pump, was 0·2436 grm.; according to the old method of decantation it was somewhat higher, namely 0·2451 grm. This excess of 1·5 milligramme shows that the adhesion of the soluble matters to the precipitate and to the filter is, in consequence of the greater pressure, more easily overcome in the new method than in the customary process; it follows, therefore, that we can obtain a more complete washing by the new method than by the old. The old process of decantation required 108 minutes and 1050 cub. centims. of water to effect a washing to the $\frac{1}{50000}$ part; the new, on the contrary, only 12 to 14 minutes, and not more than 39 to 41 cub. centims. of wash-water. If a precipitate be heated in a platinum crucible immediately after filtration by the older process, a portion will inevitably be projected out of

the crucible.   Hitherto, therefore, it has been necessary to dry
the filter and precipitate before ignition.   Now to dry a quantity
of hydrated chromium sesquioxide containing 0·2436 grm.
$Cr^2 O^3$ in a water-bath at 100° C. requires at least five hours ; and,
moreover, bringing the dried precipitate into the crucible, burn-
ing the filter, and gradually igniting the mass is in the highest
degree tedious and troublesome.   All this expenditure of time
and labour may be saved by employing the new method.   By
its means a precipitate is as completely dried upon the filter in
from 1 to 5 minutes as if it had been exposed from 5 to 8 hours
in a drying-chamber ; and it can immediately, filter and all,
be thrown into a platinum or porcelain crucible and ignited
without the slightest fear of its spurting.   By operating in the
following manner the filter burns quietly without flame or
smoke ; this phenomenon, although remarkable, easily admits of
an explanation.   The portion of filter-paper free from precipi-
tate is tightly wrapped round the remainder of the filter in such
a manner that the precipitate is enveloped in from four to six
folds of clean paper.   The whole is then dropped into the pla-
tinum or porcelain crucible lying obliquely upon a triangle over
the lamp, and pushed down against its sides with the finger.
The cover is then supported against the mouth of the crucible in
the ordinary way, and the ignition commenced by heating the
portion of the crucible in contact with the cover.   When the
flame has the proper size and position, the filter carbonizes
quietly without any appearance of flame or considerable amount
of smoke.   When the carbonization proceeds too slowly, the
flame is moved a little towards the bottom of the crucible.   After
some time the precipitate appears to be surrounded only by an
extremely thin envelope of carbon, possessing exactly the form
(of course diminished in size) of the original filter ; the flame is
then increased, and the crucible maintained at a bright-red heat
until the carbon contained in this envelope is consumed.   The
combustion proceeds so quietly that the resulting ash surround-
ing the precipitate possesses, even to the smallest fold, the exact
form of the original filter.   If the ash shows here and there a
dark colour, it is simply necessary to heat the crucible over the
blowpipe for a few minutes to effect the complete removal of the
trace of carbon.   This method of burning a filter is extremely
convenient and accurate ; it is only necessary to give a little
attention at first to the slow carbonization of the paper, after
which the further progress of the operation may be left to itself.
    Gelatinous, finely divided, granular, and crystalline precipi-
tates, such as alumina, calcium oxalate, barium sulphate, silica,
magnesium ammonium phosphate, &c., may with equal facility
be treated in this manner ; so that even in this particular the

work, in comparison with the method generally adopted, is considerably shortened and simplified.

From the above experiments it appears that the time necessary to filter and dry a quantity of chromium sesquioxide, hitherto requiring about 7 hours, is reduced by the new method to 13 minutes. This saving of time is, moreover, proportionately greater in the case of precipitates more easily filtered than hydrated chromium sesquioxide. Particularly is this so in separating a finely suspended precipitate from a large volume of water. Under these circumstances the clear fluid runs through the filter in a continuous stream, so rapidly that it is scarcely possible to maintain the supply ; the entire operation, in fact, requires scarcely more time than that necessary to pour a liquid from one vessel to another. Filtration, therefore, may be effected as quickly through the smallest as through the largest filter. Moreover the exceedingly small amount of water required to wash a precipitate completely renders unnecessary the tedious evaporations which by the older method are almost inevitable when the filtrate is needed for a further separation. Thus the introduction of impurities from the action of the liquid upon the dish in the course of evaporation is prevented ; and also the loss due to the slight solubility of the greater number of precipitates in the wash-water is reduced to a minimum. Supposing we had to analyze an alkaline chromate in which the quantity of chromic acid is equivalent to 0.2436 grm. chromic sesquioxide, as in the above described experiments, then to determine the proportion of alkali we should, by using the older method, require the preliminary evaporation of about 1050 cub. centims. of liquid; by the new method the evaporation of 40 cub. centims. only is necessary. Now by employing the best form of waterbath, *i. e.* one possessing a constant water-level, such as is used in my laboratory, it is possible, under favourable circumstances, to evaporate in a porcelain dish 1 cub. centim. of water in 27 seconds. Consequently the evaporation of the filtrate obtained by the older method would occupy about 8 hours, whilst by the new 18 minutes only are required. The total length of time needed to filter the chromium sesquioxide, wash and dry the precipitate, and evaporate the filtrate is reduced, therefore, from 14 or 15 hours to about 32 minutes.

The experience I have subsequently gained in my laboratory, where the method has been in general use for the last nine months, fully confirms the above results. It has shown that, on the average, three or four analyses can now be made in the time formerly demanded by a single one.

Another and an inestimable advantage springs from the peculiar condition of a precipitate filtered by this method. It not

unfrequently happens, even in the hands of experienced mani-
pulators, in consequence of the agitation it is necessary to give
to the contents of the filter to effect their complete washing,
that the surface of the filter becomes injured and torn, so that
the precipitate becomes mixed with filaments of paper ; this is
particularly the case in using hot water. Supposing the preci-
pitate to consist of mixed hydrates of the sesquioxides (for ex-
ample, iron and alumina), it will be found, on redissolving in an
acid, that the filaments, like tartaric acid, prevent the complete
separation of these substances by subsequent precipitation ; thus
the alumina will contain iron, and on precipitation by means of
ammonium sulphide will be coloured black.   On the other
hand, by employing the new method the precipitate coheres so
firmly that the introduction of this source of error is impossible,
even by using common grey filter-paper.   The most gelatinous
precipitates, as hydrated ferric oxide, alumina, &c., adhere to the
filter in a thin coherent layer, and may be removed, piece after
piece, so completely that the paper remains perfectly clean and
white.   The advantage thus gained where it is necessary to
transfer mixed precipitates to another vessel in order to effect
their subsequent separation is evident.

The filter-pump, moreover, is exceedingly serviceable in sepa-
rating precipitates or crystals from syrupy mother-liquors. Thus
honey-sugar may be so completely separated from the thick
viscid liquid in which it forms by a filter of coarse grey paper,
that it remains only slightly coloured, and by a single crystalli-
zation from alcohol may be obtained in small white shining
needles.   And since the bulk of the moist precipitates, particu-
larly that of the more gelatinous, is so much diminished under
the high pressure, the precipitate only occupying one-third to
one-sixth of its bulk under ordinary circumstances, a filter of
one-third to one-sixth of the size usually employed may be
taken, and thus the amount of ash proportionately lessened.

As the water air-pump suffers no injury from the presence of
corrosive vapours or gases, we can equally well employ it to filter
liquids containing nitrous acid, sulphurous acid, fuming nitric
acid, chlorine, bromine, volatile chlorides, &c.   In such cases I
use a peculiar filtering arrangement, consisting of a cylindrical
glass vessel, the lower end of which is drawn out before the
blowpipe to the form shown in fig. 5 ; in this drawn-out portion
a thin plate, 1 or 2 millims. in thickness, of *artificial* pumice,
such as is used by polishers, is packed water-tight by means of
asbestos.   This apparatus is arranged for the purpose required
exactly as the funnel in the method of filtration by pressure
above described.   In order to have a number of these filters in
readiness, a pumice-stone cylinder of the required diameter is

turned in a lathe, and then the thin plates sawn off by means of a small hand-saw in the small wooden support shown in fig. 6. The upper surfaces of the plates may afterwards be rendered perfectly even by a coarse file.

By the aid of these pumice-stone filters many chemical products may be made, the preparation of which has hitherto been almost impossible. For the sake of example I take the preparation of pure dry chromic anhydride; in an hour it is easily possible to filter, wash, and dry crystals of this substance an inch in length. A solution of 2 parts of potassium dichromate in 20 parts of water mixed with 10 parts of concentrated sulphuric acid, deposits, after standing about 24 hours, numerous brilliant needles of chromic anhydride. These may be drained from adhering mother-liquor upon the pumice filter by means of the pump, and in a few minutes completely washed by a small quantity of fuming nitric acid free from nitrous acid. A covering of tolerably strong sheet copper provided with two arms, as shown in fig. 5, is then placed round the tube; by hanging lamps upon the arms the tube may be readily heated to about 60° or 80° C.; and by connecting a chloride-of-calcium tube with the upper end of the glass vessel, a current of dry air may be drawn through the apparatus by means of the pump, and thus in a comparatively short time large and brilliant crystals of chromic anhydride, perfectly dry and free from all impurity, may be easily obtained.

A single pump of the above description costs, including the leaden piping, about 8 thalers (24 shillings); and experience has shown that five or six are amply sufficient for a laboratory of fifty or sixty students. The apparatus, as may readily be seen, can be applied in the operation of evaporating *in vacuo*; if, however, circumstances will not permit of its being adapted to this purpose, then a fall of 10 or 15 feet is sufficient to filter a precipitate according to the above described method, and so far to dry it that it can be immediately ignited in the crucible. It is therefore not absolutely necessary to employ an air-pump in this process of filtration; any apparatus producing a difference of pressure amounting to a quarter of an atmosphere is sufficient. The simple arrangement represented in fig. 7 is very useful, and is frequently employed in my laboratory. It consists of two equal-sized bottles, a and $a_{\prime\prime}$, of from 2 to 4 litres capacity, each of which is provided near the bottom with a small stopcock designed to regulate the flow of water. Suppose a filled with water and placed upon a shelf as high above the ground as possible and $a^\prime$ placed empty on the floor, and the two stopcocks connected by means of caoutchouc tubing c, then on allowing water to flow down the tube the air in the upper bottle be-

comes somewhat rarefied; and in order to employ the consequent difference in pressure (amounting to a column of mercury about 0·2 metre in height) for the purpose of filtration, it is only necessary to connect the mouth of the upper bottle with the tube of the filter-flask. When the water has ceased to flow, the position of the bottle is reversed, when the operation recommences. So small a pressure as 0·2 metre suffices to render the filter and its contents so far dry that they may be immediately withdrawn from the funnel and ignited without any other preliminary desiccation. The following experiment, made with a portion of the same solution of chromium used in the former determinations, will serve to show the saving of time effected by this simple arrangement :—

<div align="center">

XI.

</div>

Transferring the precipitate with 14 cub. centims. of water	14
For a single addition of 26 cub. centims. of wash-water to run through	7
To drain the precipitate	4
Time required in washing	25

Weight of the precipitate	0·2435 grm.
Volume of wash-water	40 cub. centims.
Pressure in manometer	0·184 metre.

This amount of chromium sesquioxide (0·2435 grm.) differs from the mean of the former experiments (0·2436 grm.) by one-tenth of a milligramme only, and shows that even by a pressure of 0·184 metre the washing is as complete by the single addition of 26 cub. centims. of water. The duration of the filtering process in the former experiments ranged from 12 to 14 minutes under a difference of pressure amounting to from 0·53 to 0·572 metre; in the last experiment it required 25 minutes under a pressure of 0·184 metre, or about double the length of time. The time needed to analyze potassium chromate in the former case was reduced from 14 hours to 32 minutes; by the latter method the reduction would be from 14 hours to 44 minutes.

The employment of the second method is particularly to be recommended to beginners in qualitative analysis. The experimenter needs only a single funnel, he is obliged to work carefully and cleanly, and the great saving of time and work amply compensates for the little trouble needed to reverse from time to time the position of the bottles.

I believe that the above-described water air-pump will soon become an indispensable piece of apparatus in chemical laboratories. It not only serves as the most convenient method of

producing the differences in pressure required to accelerate the process of filtration, and of obtaining the necessary vacuum for evaporation ; it is equally adapted to purposes to which neither the mercury nor the ordinary pumps are in any way applicable. By its aid it is possible to calibrate a thermometer with the greatest accuracy, and to estimate the vapour-tension of such corrosive bodies as bromine, chromyl dichloride, &c. by the simplest method possible, in which the necessary operations require scarcely more time than an ordinary determination of a boiling-point.

I purpose returning to these applications of the instrument in a future communication.

II. *On a New Form of Permanent Magnet.* By FREDERICK A. PAGET, *C.E. ; M. Soc. Civil Engineers of France ; Corr. Mem. Franklin Institute ; M. late Government Commission on Chaincable- and Anchor-proving Establishments**.

WITHOUT any distinctly given reason, it is taken for granted in all works on magnetism, and in all the practical applications of magnetism, that it is impossible to magnetize a plate except in the direction of its greatest length.

Michell, in his 'Treatise on Artificial Magnets,' gives a determinate proportion, but without stating any reason, between the length and the weight of magnets. A magnet, for instance, 2 inches long should weigh one-tenth of a pound. Cavallo recommends a width of one-tenth, Fuss one-sixth, Musschebroeck and, later, Coulomb one twenty-fourth, of the length. In all these cases it is assumed that the direction of the poles must be parallel with the longest dimensions of the solid bar or plate to be magnetized, and that it is impossible to regularly magnetize a square plate, and still less an oblong plate, in a direction transverse to its major axis. That this is correct with a solid continuous plate can be easily proved by experiment ; and it is well known to instrument-makers that it is impossible to *permanently* magnetize a square steel plate. No doubt such results would greatly vary with the constitution and state of the steel employed, the relations of its different dimensions, the mode of magnetization adopted ; but the only experiment bearing on the question that I can discover, after much research in scientific works, is that of De la Borne†, who found, on magnetizing steel disks, that as long as they were whole they showed no polarity, and that their polarity only appeared when they were cut in two. Dr. Lamont, in a paper which first appeared in Poggendorff's *Annalen* (vol. cxiii.), and was communicated to the Philosophical

* Communicated by the Author.
† *Pogg. Ann.* vol. lxxii. p. 26.

Magazine for November 1861 by the Astronomer Royal, investigated the question of "the most advantageous form of magnets," or that form in which "are united the greatest possible magnetic moment with the smallest possible mass and the smallest possible moment of inertia." In all the forms he experimented upon, the breadth was always less than one-third of the length, and generally about one-fifth; and he does not seem to contemplate the possibility of magnetizing a square plate, and still less an oblong plate, in a direction transverse to its greatest length. Now I find that, by cutting slits nearly up to the middle of a steel plate, a square plate in one piece can with such slits be regularly magnetized; and by this means even an oblong square plate can be regularly magnetized, and with as many poles as may be required, in a direction transverse to its greatest length. I herewith beg to forward a square plate magnetized in this way. It is of watch-spring steel, 0·0075 inch thick and $\frac{7}{8}$ inch × $\frac{7}{8}$ inch; it has four pairs of slits $\frac{1}{8}$ inch wide cut from its edges, and leaving a central web $\frac{1}{4}$ inch wide, uniting the whole. On moving a small needle round this square plate, it is seen to be regularly magnetized; and on sprinkling iron filings on the magnet covered by a sheet of paper, they arrange themselves in lines, proving that the magnet really consists of a number of small regular similar magnets arranged below each other in the same vertical plane. On suspending an oblong magnet of this kind with its longer axis in the vertical plane, the needles set themselves to the magnetic meridian; on suspending it flatwise, with its longer axis in the horizontal plane, the longer axis points east and west. As well as can be judged by subjecting them to slight shocks, the magnets are as permanently magnetized as if they were separate from each other. Only time can prove whether they will lose their magnetism. The important question as to what form is the best for retaining magnetism for a length of time is one which, as Dr. Lamont remarks, no one has yet investigated.

Though I have not yet been enabled to try to magnetize a parallelopipedon of steel after slotting it vertically and transversely into a number of bars held together by a central web, I feel very confident that this could be done. The slots could be cut into the parallelopipedon or cube while in a soft state by a thin tool worked to and fro in an ordinary engineers' slotting or shaping machine, and the whole magnetized in a powerful electric spiral, in the way described by Elias. Besides moving the spiral to and fro, as described by him, no doubt in order to overcome the resistance to induction, the cube while in the spiral could also be struck, in order to produce that mechanical vibration which is so favourable to magnetization and demagnetization.

By pointing the needles, or giving them the rhomboidal form, it is evident that, in spite of the poles being nearer (as is well ascertained to be the case in needles of that shape), the ratio of the magnetic moment to the moment of inertia is still higher than in the assemblage of oblong needles. Though, for various reasons, the rhomboid set on its edge is scarcely ever used in practice, an easy calculation shows that it is the most perfect form for a moveable magnet. It seems only to have been tried when lying flat.

Seymour Chambers, Adelphi, W.C.
London, November 20, 1868.

III. *Action of Dehydrating Agents on Organic Bodies.* *By* E. Theophron Chapman and Miles H. Smith[*].

CANE-SUGAR, as is well known, yields carbon on treatment with strong sulphuric acid, which removes the elements of water. In like manner strong sulphuric acid dehydrates common alcohol, yielding olefiant gas. Chloride of zinc removes water from amylic alcohol, yielding amylene. Anhydrous phosphoric acid converts acetate of ammonia into acetamide, and finally into cyanide of methyle. All these are well-known and characteristic examples of the violent dehydration of organic substances. The three short notices which follow describe fresh cases of the action of dehydrating agents.

The first is the instance of nitrate of amyle with phosphoric acid : there is very violent dehydration, and pyridine is produced,

$$C^5 H^{11} NO^3 - 3 H^2 O = C^5 H^5 N.$$

The second is the dehydration of formiate of amylamine by means of chloride of zinc : there is produced *iso*-cyanide of amyle,

$$CH^2 O^2 N C^5 H^{13} - 2 H^2 O = C N C^5 H^{11}.$$
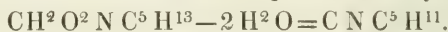
The third is a case in which a strong dehydrating agent refused to perform a *violent* dehydration, viz. the action of chloride of zinc on oxalate of amyle. The equation

$$C^2 O^4 (C^5 H^{11})^2 - 4 H^2 O = C^{12} H^{14}$$
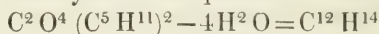
was *not* realized.

That which actually did happen was as follows :—

$$C^2 O^4 (C^5 H^{11})^2 - H^2 O = C^2 O^3 + 2(C^5 H^{10}).$$
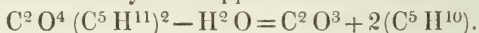
I. *On the Artificial Production of Pyridine.*

Pyridine has been produced by Perkin from azo-dinaphthyldiamine ($C^{20} H^{15} N^3$) by the action of nascent hydrogen. This

reaction has not thrown much light on the structure of the base in question. The other sources of this body are the destructive distillation of various nitrogenous matters. It is found in bone-oil and in the distillate from peat, shale, &c., but in no case is the rationale of its formation understood.

In a paper read before the Chemical Society and published in its Journal for August 1866, entitled "On the Production of Acetic and Propionic Acids from Amylic Alcohol" (Chapman), occurs this passage:—"Phosphoric acid attacks nitrates as well as nitrites, though not so easily, and this may prove a method of obtaining compounds of the same class as picoline.

$$C^5 H^{11} NO^3 \text{ might give us } C^5 H^5 N + 3H^2 O."$$

Nitrate of amyle.   Pyridine.
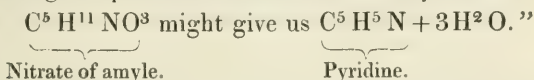
This hypothetical equation has been realized exactly in the manner indicated.

If excess of dry nitrate of amyle be poured upon anhydrous phosphoric acid, at first no reaction appears to take place; but on long standing or on the application of gentle warmth, the phosphoric acid is seen to shrink a little and to change in appearance. This change generally commences at one side of the vessel, and gradually creeps through the mass. Considerable heat is produced, but no gas evolved. On treating the mixture with water no further heat is generated; but the solid mass in the vessel gradually dissolves, the excess of nitrate separates and may be decanted off, and the last traces of it may be completely removed by a few minutes' boiling. On now adding excess of potash to the liquid, the smell of pyridine is at once produced. But though we operated on several ounces of anhydrous phosphoric acid, we could not in this manner obtain more than the slightest traces of the base. A good deal of dark-brown pitchy matter, possessing apparently feeble basic properties, is the chief product of the reaction. This substance is probably intermediate in composition between pyridine and nitrate of amyle. Acting on this view, we determined to use excess of anhydrous phosphoric acid. We here, however, encountered another difficulty, the reaction between anhydrous phosphoric acid and nitrate of amyle being, under these circumstances, uncontrollably violent, though it does not occur until some time after the substances have been placed in contact, or unless they have been gently warmed. We found the only available method of operating was to take a very long-necked flask, and place in it between two and three grammes of anhydrous phosphoric acid, together with from one and a half to two grammes of nitrate of amyle. The mixture was carefully spread out in a thin layer in the flask. During this operation it is necessary to keep the flask cool by

surrounding it with iced water. A cloth dipped in iced water is now wrapped round the neck of the flask, and the bulb placed in the water-bath for a few minutes until the first signs of reaction are visible. It is then removed from the water-bath and wafted about in the air. As soon as the chief part of the reaction is over, the flask is replaced in the water-bath for a few minutes. This process is repeated with fresh supplies of material in clean flasks. The contents of the flasks are now dissolved in water, and the solution so obtained distilled with excess of potash. This solution is very dark in colour. The alkaline distillate, which smells strongly of pyridine, is rendered acid with sulphuric acid, and boiled to expel traces of neutral oily matters. It is then considerably concentrated by evaporation in the water-bath, and finally pieces of potash are added. The oily liquid which rises to the surface is pyridine. It was recognized to be such by its extreme stability, by its odour, and by an analysis of its chloride, the details of which are subjoined.

Substance taken,

$$·4024.$$

Chloride of silver found,   ·4992 ;

therefore percentage of Cl,   30·69 ;

($C^5 H^5 NHCl$) requires   30·73.

The above process does not yield the full theoretical quantity of pyridine. Some dark-coloured neutral or slightly alkaline body is produced in large quantity.

II. *Organic Cyanides.*

The question of isomerism amongst the cyanides of the alcohol-radicals, or nitriles, has acquired a great interest since the publication of Hofmann's recent researches on the subject. We must now recognize two distinct kinds of isomerism amongst the nitriles :—first, isomerism dependent on the linking of the carbon *inter se* ; second, isomerism dependent on the atomicity of the nitrogen.

As an example of the first kind of isomerism, we may mention cyanide of isopropyle and cyanide of normal propyle or butyro-nitrile. This kind of isomerism can obviously exist in nitriles independently of the atomicity of the nitrogen.

We may represent cyanogen in three ways :—

(1) $N'''\ C'''\ C''''\ N'''$. In this case the two equivalents of cyanogen which constitute the molecule are held together by the carbon

(2) $C'''' \; N'''''' \; N'''''' \; C''''$.   In this case the equivalents are held together by the nitrogen.

(3) $C'''' \; N'''''' \; C'''' \; N'''$.   In this case the equivalents are held together by one carbon and one nitrogen.

If the first of these assumed compounds were to be combined with a monatomic body, it would yield a compound (M = monatomic body) of the following constitution, $N''' \; C'''' \; M$, the monatomic body being attached to the C. The second would yield $C'''' \; N^{''}_{''} \; M$, " M " being attached to the nitrogen. The third would obviously yield a mixture of the first two in atomic proportions. By assuming the existence of the first two of these different forms of cyanogen, we are enabled to give an account of the difference between the two classes of nitriles ; and we think that some grounds exist for making this assumption.

We find that the action of the iodides of the alcohol-radicals on different metallic cyanides produce different nitriles. For example, as Meyer showed many years ago, the cyanide of ethyle produced by the action of cyanide of silver on iodide of ethyle yields ethylamine when treated with an acid. Hofmann has shown that the complementary product in this reaction is formic acid ; and we, who have also examined this action, can corroborate the observation. Again, cyanide of mercury under some circumstances may be made to yield both of the isomeric cyanides by the action of the alcoholic iodides. We have not completed the examination of this reaction. A volatile mercury compound is formed at the same time. As Hofmann has pointed out, the isomeric nitriles are reproduced when the sulphovinic salts act on the metallic cyanides, though not in quantity.

The above facts appear explicable only on the hypothesis of two distinct cyanogens, or, in other words, on the assumption that in some cases cyanogen unites by its carbon and sometimes by its nitrogen. The two cyanides of ethyle would then be represented thus :—

(1)   Common cyanide, $N''' \; C'''' \; C^2 \; H^5$.

(2)   Pseudo-cyanide, $C'''' \; N^{''}_{''} \; C^2 \; H^5$.

In the first case the carbon of the cyanogen is in union with the carbon of the ethyle, and remains so when the cyanogen undergoes its typical alteration by the assimilation of water.

In the second case the carbon of the cyanogen is *not* in union with the carbon of the ethyle, and therefore, when the cyanogen undergoes its transformation by assimilation of water, it does not combine with the ethyle.

The methods for the production of the common cyanides are three :—

(1) Action of an organic iodide, bromide, or chloride on a metallic cyanide.

For example, $K C'''' N''' + C^2 H^5 I = C^2 H^5 C N + KI$.

(2) Action of sulphovinates on metallic cyanides.

For example,

$$SO^4 \left\{ \begin{matrix} K \\ C^2 H^5 \end{matrix} \right. + K C'''' N''' = C^2 H^5 C N + SO^4 \left\{ \begin{matrix} K \\ K \end{matrix} \right. .$$
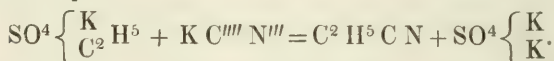
(3) Dehydration of the ammonia-salts, or the amides of the acids of the same carbon condensation as the cyanide produced. For example,

Propionate of ammonia. $C^3 H^6 O^2 N H^3 - 2 H^2 O = C^2 H^5 CN$.

Propionamide   .   .   . $C^3 H^5 O N H^2 - H^2 O = C^2 H^5 CN$.

The first two of these methods may also give rise to the so-called pseudo-cyanides ; or perhaps we should say that analogous operations on differently constituted metallic cyanides give rise to the pseudo-cyanides. It proves nothing against this hypothesis that one and the same metallic cyanide may give rise to both, or now one and now the other of the cyanides; for the metallic cyanides themselves may be very liable to change their internal arrangement in accordance with external circumstances.

The third process gives us but one cyanide—the normal one.

The question naturally arises, is there any analogue of this last process which will give rise to the pseudo-cyanides ? Perhaps Hofmann's method for the preparation of these bodies may be regarded as to a great extent analogous. But we find that there is a much more strictly analogous process; for by dehydrating the formiates of the amides we obtain the pseudo-cyanides. The first step in the dehydration of the formiates of the amides is the difficulty ; but by starting with the formamides no difficulty is experienced.   Nor is there the slightest difficulty in obtaining the compound formamides ; they are produced with remarkable ease by digesting the compound ammonias with formic ether.   By digesting aniline with formic ether in excess at 130° C. for a few hours, it is completely converted into phenyl-formamide. Amylamine is converted into amylformamide, even in the water-bath, when digested with formic ether.   Ethylamine is even more rapidly converted. A little alcohol facilitates the reaction.   These compound formamides may be purified sufficiently by simply heating them to a temperature rather above that of the water-bath for a short time.   If after this treatment they be treated with chloride of zinc (fused and in coarse powder), the pseudo-cyanides are at once produced, and may be recognized by their smell and by their decomposition with acids.

This process can hardly be said to be an available method of

obtaining these cyanides in quantity; or, rather, it is nothing like so available as Hofmann's process with chloroform. After obtaining these cyanides, we attempted to obtain analogous bodies from the compound acetamides, but without success. Action took place fast enough; but charring and other splitting up occurred.

Anhydrous phosphoric acid, by its action on the compound formamides, also produces the pseudo-cyanides.

The conclusion we draw from the whole of the foregoing is that the pseudo-cyanides have as much right to be regarded as cyanides as the common cyanides, and that we must look for the difference between these classes of cyanides in differences with cyanogens themselves.

III. *Action of Chloride of Zinc on the Oxalic Ethers.*

The numerous interesting reactions which the oxalic ethers are known to undergo rendered it probable that the action of dehydrating agents on these ethers would be worth studying. We have therefore treated three of the oxalic ethers with chloride of zinc. These ethers were the oxalates of methyle, ethyle, and amyle. We expected either that an actual dehydration of the ethers would take place at the expense of the oxygen of the oxalic acid and hydrogen of the alcohol radical, or that, simply, double decomposition would take place, and that we should obtain oxalate of zinc and chloride of the alcohol radical. Neither of these two results ensued.

Chloride of Zinc and Oxalate of Ethyl.—When these substances are heated together, torrents of gas are evolved. This gas is partially soluble in water. That portion of it which is not soluble in water is completely and readily soluble in bromine. It burns with a luminous flame, and is in fact ethylene. The soluble gas is hydrochloric acid. Oxalate of zinc is the solid product of the reaction. The reaction consists, therefore, in a double decomposition of oxalic ether and chloride of zinc; but instead of obtaining chloride of ethyle, we have its elements in the form of hydrochloric acid and ethylene.

Chloride of Zinc and Oxalate of Amyle.—The reaction in this case is exactly similar to the one above described, excepting that the amylene produced is for the most part polymerized.

Chloride of Zinc and Oxalate of Methyle.—When these substances are heated together, gas is evolved as before, in large quantities. When this gas is passed through ice-cold water, a very considerable portion of it is condensed, and an oily layer of liquid is formed on the surface of the water. On conducting the non-condensed gases through a U-tube surrounded with ice and salt, some more light liquid condenses. There is, however,

still a residue of gas. This gas is partially absorbed by bromine, not entirely. The residue appears to be chloride of methyle. The liquids condensed both on the surface of the water and in the U-tube were obviously for the most part olefines. They began to boil below zero (Cent.), and probably boil at all temperatures up to about 60°, when the distilling vessel was found to be dry. They appeared to be a mixture of various olefines. This reaction apparently resembles closely that with the oxalate of amyle.

London Institution.

IV. *On the Explanation of* Stewart *and* Tait's *Experiments on the Heating of a Disk rotating in a Vacuum.* By OSCAR EMIL MEYER[*].

IN a previous paper[†] I have already briefly discussed the experiments on the heating of a disk in an exhausted space which Messrs. Stewart and Tait laid, in June 1865, before the Royal Society of London[‡]; and I then expressed the opinion that the agitations which are communicated to the rotating disk by the wheelwork are the chief cause of the heating. I revert to the subject once more, because the gentlemen in question have published another paper on the subject[§], in which it is proved that the cause of the heating is to be sought neither in terrestrial magnetism, nor in conduction, nor radiation of heat, nor in the surrounding air. I think, as I have already said, that the cause lies in the agitations caused by the wheelwork.

I should not consider it worth while to elucidate this subject to the readers of the *Annalen*, if, with the aid of the explanation in question, the experiments did not enable us to calculate *the coefficient of thermal radiation of the disk in absolute measure.* We find in this manner a number which agrees most completely with a formula which Professor Neumann, of Königsberg, has most kindly communicated to me. This formula is based on the observations of Dulong and Petit on the law of cooling[||], and an observation which he himself has made. The agreement between the results obtained in these different ways affords the conviction not only that the explanation of Stewart and Tait's observation is correct, but also that the value in absolute measure obtained for the thermal radiation is undoubtedly accurate.

The agitations to which I ascribe the heating are communicated to the disk by the wheelwork ; they are due to slight irregularities in the working of the axes and wheels, and are such that the rotating disk and its axis, within the play left to it, is continually moved backwards and forwards.

Such an oscillation cannot escape observation ; for the radius of the disk amounts to $6\frac{1}{2}$ inches = 165 millims. If, therefore, the axis (which is certainly far shorter) moves only the hundredth of a millimetre in its bearings, there must be a shaking of the edge, and in rapid rotation an apparent increase in the thickness, of the disk.

Stewart and Tait have, it is true, noticed this phenomenon ; they observed a rising and sinking of the aluminium disk used (which was $\frac{1}{20}$ inch thick) of 0·015 inch, or 0·38 millim., on both sides of the edge*. They explain this, it appears, on the assumption that disk and axis were not fastened to each other exactly at right angles. I consider it not less probable that an oscillation of the axis was the cause.

If, however, this assumption is correct, it is a necessary consequence that the oscillation must be the stronger the lighter the disk. This, in fact, was noticed by Stewart and Tait ; for they found that while the disk of $\frac{1}{20}$ inch thickness deviated by 0·015 inch, that which was half as thick moved up and down as much as 0·02 inch.

It follows, moreover, from this assumption that the *vis viva* which was communicated to the disk by the wheelwork must have been the same in both cases. The quantities of heat resulting from these equal *vires vivæ* must have been equal ; that is, the one of half the thickness must have been twice as hot as the one which was double. This, however, is exactly what has been observed by Stewart and Tait†.

After this confirmation of the hypothesis, it seemed worth while to calculate the magnitude of the *vis viva* which is changed into heat by agitations and impulses.

In this calculation we are concerned both with the *number* of the impulses and with their *strength*.

Since the wheelwork runs with constant velocity, the impulses occur regularly. The axis of the disk rolls, therefore, with regularity within the space which its ends have on their bearings. The axis describes a kind of conical surface. After each revolution it comes into the same position, or, at all events, into almost the same position ; after each half revolution, into the opposite one. During each revolution, therefore, it is thrown once forward and once backward ; or during each turn it experiences two impulses which change its position and direction.

* Article 20 (2).   † Experiments XIII. and XX. Article 18.

At every impulse upon the axis one part of the *vis viva* present is lost; for at each impulse the position of the axis of rotation is changed; hence, of the *vis viva* present, only that part remains which corresponds to a rotation about the new axis; all the rest of the *vis viva* is lost, as far as rotation is concerned, and is used in heating the disk.

From this we can easily calculate the loss of *vis viva* and the gain in heat occurring in each second. If we denote the angular velocity of the disk by ψ, the *vis viva* of the particles at the distance r from the axis is

$$\tfrac{1}{2} r^2 \psi^2$$

for the unit of mass.

At this distance, however, there is an infinitely narrow zone of the breadth dr, and the thickness of the disk δ, which contains the mass

$$2\pi r \delta \Delta dr,$$

if Δ denotes the density of the disk of aluminium. This zone has therefore the *vis viva*

$$\pi \delta \Delta \psi^2 r^2 dr ;$$

and the entire disk the integral of this expression,

$$\frac{\pi}{4} \delta \Delta \psi^2 R^4,$$

where R denotes the radius of the disk, or

$$\tfrac{1}{4} M R^2 \psi^2$$

by introducing the mass of the disk,

$$M = \pi R^2 \delta \Delta.$$

If, now, owing to one of the impulses in question, the axis of the disk is deviated through the angle ψ, the residual *vis viva* thereby becomes

$$\tfrac{1}{4} M R^2 \psi^2 \cos^2 \phi,$$

and that which is lost for rotation and changed into heat is

$$\tfrac{1}{4} M R^2 \psi^2 \sin^2 \phi.$$

This loss of *vis viva* and gain in heat occurs twice during each rotation—in the unit of time $\frac{2}{T}$, if T denotes the time of one rotation of the disk. The heat produced, therefore, in the unit of time is equivalent to the *vis viva,*

$$\frac{1}{2T} M R^2 \psi^2 \sin^2 \phi;$$

or, since

$$\psi = \frac{2\pi}{T},$$

it is

$$\frac{2\pi^2}{T^3} M R^2 \sin^2 \phi.$$

In this expression $R \sin \phi$ has a simple meaning; for it is nothing more than the magnitude of the alternate rising and sinking of the edge of the disk R, the value of which is 0·015 inch or 0·38 millim. By introducing this value the loss of *vis viva* may also be written

$$\frac{2\pi^2}{T^3} M k^3.$$

This *vis viva* of motion changed into heat is first of all consumed in raising the temperature of the disk, and is then imparted to the surrounding medium by radiation. Since after some time both the velocity of the rotation and also the excess of the temperature of the disk over that of the surrounding medium became constant, the heat lost in a second by radiation must be equivalent to *vis viva* transformed into heat during the same time. The first may be calculated from Newton's law of cooling, which, owing to the small amount of the heating, may be unhesitatingly accepted. If the constant excess of the temperature of the disk amounts to t degrees, the quantity of heat radiated in a second from both surfaces of the disk is

$$2\pi h R^2 t,$$

if the constant h denotes the heat which is radiated by the unit of surface for an increase of 1 degree. I obtain the mechanical work equivalent to this heat by multiplying by Qg, where g is the accelerating force of gravity, and Q the height to which the unit of mass can be raised by the unit of heat. The equivalent in work of that heat is therefore

$$2\pi h R^2 t Q g.$$

The work thus produced corresponds to the *vis viva* consumed —that is,

$$h R^2 t Q g = \frac{\pi}{T^3} M k^3.$$

The first idea suggested by an inspection of this formula is a circumstance which apparently disagrees with observation. For Stewart and Tait have observed that the heating of the disk is inversely proportional to its thickness. From the above equation we might be tempted to conclude that the heating t increases proportionally to the mass M, and therefore also to the thickness of the disk. We must, however, remember that the oscillation must be the greater the less the thickness of the disk*. The os-

* It is true that the above numbers do not accurately confirm this; but they are only approximate measurements.

cillation k is therefore inversely proportional to the thickness,
and it follows that the heating of the disk must also increase
inversely as the thickness.

All the magnitudes occurring in the formula are known from
Stewart and Tait's measurements, or may easily be calculated
from them, including the constant h which defines the thermal
radiation. No direct statements have been published; so much
the more interesting, therefore, does it appear to deduce their
value from the observations in question.

If we introduce into the above formula the numerical values

$$k = 0\cdot015 \text{ inch} \quad = 0\cdot38 \text{ millim.},$$
$$M = 10 \text{ ounces} \quad = 310 \text{ grms.},$$
$$R = 6\cdot5 \text{ inches} \quad = 165 \text{ millims.},$$
$$T = \frac{30''}{2500} = 0''\cdot012,$$
$$Q = 423\cdot5 \text{ metre},$$

and for g and π their well-known values; and if, finally, we
assume for the heating the mean value

$$t = 0^\circ\cdot84 \text{ F.} = 0^\circ\cdot46 \text{ C.},$$

which holds for the disk coated by lampblack, we get the ther-
mal radiation

$$h = 0\cdot0017.$$

This number contains no arbitrary unit of heat, but is con-
nected solely with the so-called absolute units (that is, the milli-
metre and the second of time), as well as the density of water as
unit of specific gravity. It stands as $\mathfrak{S}$ for a surface blackened
by lampblack in a rarefied space in which there is a tension of
$0\cdot3$ inch or $7\cdot6$ millims. of mercury.

An idea is obtained of the meaning of the number thus
found by considering that a blackened surface of 1 square metre,
which has been heated 1 degree above the surrounding rarefied
air, loses in a second a quantity of heat which would raise a kilo-
gramme through $0\cdot72$ metre.

The value found for the thermal radiation h is in remarkable
agreement with the result which, with the kind aid of Professor
Neumann of Königsberg, I was able to deduce from the obser-
vations of Dulong and Petit. I take this opportunity of thank-
ing him publicly.

Those philosophers have combined the results of their obser-
vations on the cooling of a heated body in a rarefied space, in
the law that the quantity of heat emitted in the unit of time by
the unit of surface is expressed by the formula

$$ma^{\mathfrak{S}}(a'-1) + np^c t^b.$$

In this formula ϑ denotes the temperature of the surrounding medium, t the excess of the temperature of the heated body, p the pressure of the surrounding air; the other signs denote constants.

From the formula adduced, the value of the coefficient of thermal radiation is obtained by division by the value t, assumed to be very small. We have thus

$$h = ma^{\vartheta}\,\frac{a^t-1}{t} + np^c t^{b-1}.$$

From Dulong and Petit's determination we have, in Centigrade degrees,

$$a = 1\cdot0077,$$
$$b = 1\cdot233,$$
$$c = 0\cdot45.$$

I have to thank Professor Neumann for the statement that for a blackened surface

$$m = 3\cdot6\,;$$

and the coefficient n, which is independent of the nature of the surface, in case p is expressed in atmospheres, is for atmospheric air

$$n = 0\cdot0168.$$

The first number is deduced from his own observation, the latter from occasional statements of Dulong and Petit. Both numbers refer to Paris lines and minutes as units.

Using these values, and taking from Stewart and Tait's observation

$$p = 0\cdot010 \text{ atmosphere,}$$
$$t = 0°\cdot45 \text{ C.,}$$

and putting $\vartheta = $ about $20°$ C., we get from the above formula the value

$$h = 0\cdot0013,$$

expressed, again, in millimetres and seconds of time.

The concordance of the value deduced above from Stewart and Tait's observation with this directly found is greater than was to be expected from the multifold uncertainty of the observations.

Another beautiful agreement is also met with. According to a communication of Professor Neumann, for a metallic surface

$$m = \text{ about } 0\cdot5,$$

that is, about one-seventh that of a lampblack surface. We get from this for the radiation-constant h of a metallic surface the value

$$h = 0\cdot00023\,;$$

and it is therefore found that the radiation-coefficient for metals is about five- or sixfold smaller than that for a blackened surface. Stewart and Tait observed that the aluminium disk with a pure metallic surface radiated about one-fourth the heat of a blackened one.

Breslau, September 11, 1868.

V. *On* Hansen's *Theory of the Physical Constitution of the Moon.* *By* Simon Newcomb*.

THE great reputation of the author has given extensive currency to the hypothesis put forth by Professor Hansen some years since, that the centre of gravity of the moon is considerably further removed from us than the centre of figure. The consequences of this hypothesis are developed in an elaborate mathematical memoir to be found in the twenty-fourth volume of the Memoirs of the Royal Astronomical Society. But the reception of the doctrine seems to have been based rather on faith in its author than on any critical examination of its logical foundation†. Such an examination it is proposed to give it. An indispensable preliminary to this examination is a clear understanding of what the basis of the doctrine is. Let us then consider these three propositions:—

(1) The moon revolves on her axis with a uniform motion equal to her mean motion around the earth.

(2) Her motion around the earth is not uniform, but she is sometimes ahead of and sometimes behind her mean place, owing both to the elliptic inequality of her motions and to perturbations.

(3) Suppose her centre of gravity to be further removed from us than her centre of figure, and so placed that, when the moon is in her mean position in her orbit, the line joining these centres passes through the centre of the earth.

Let us also conceive that these two centres are visible to an observer on the earth. Then a consideration of the geometrical arrangements of the problem will make it clear that when the moon is ahead of her mean place the observer will see the two centres separated, the one nearest him being further advanced in the orbit; while, when the moon is behind her mean place,

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* From Silliman's American Journal for November 1868.

† In this connexion it is curious to notice that on page 83 of his memoir Hansen appears as the first of the independent modern discoverers of Cagnoli's theorem of spherical trigonometry—

$$\cos a \cos b \cos C + \sin a \sin b = \cos A \cos B \cos c + \sin A \sin B.$$

This was about three years before the above formula was published as new by Mr. Cayley, and geometrically demonstrated by Professor Airy, in the Philosophical Magazine.

the nearest centre will be behind the other. This apparent oscillation of the two centres is indeed an immediate effect of the moon's libration in longitude.

Now the inequalities in the moon's motion, computed from the theory of gravitation, are those of a supposed centre of gravity. But the inequalities given by observation are those of the centre of figure. Hence, in the case supposed, the inequalities of observation will be greater than those of theory. Also their ratio will be inversely as that of the distances of the centres which they represent.

Professor Hansen, in comparing his theory with observations, found that the theoretical inequalities would agree better with observation when multiplied by the constant factor $1\cdot0001544$. Supposing that this result could be accounted for on the hypothesis of a separation of the centres of gravity and figure, he thence inferred that the hypothesis was true. But the result cannot be entirely accounted for in this way, because the largest inequality of theory (evection) has a factor (excentricity) which can only be determined from observation; and therefore even the theoretical evection is that of the centre of figure, and not of the centre of gravity. It must not be forgotten that the excentricity, which is not given by theory, is subject to be multiplied by the same factor that multiplies the other inequalities. To be more explicit,—

Let e be the true excentricity of the orbit described by the moon's centre of gravity. Then the true evection in the same orbit will be

$$e \times A,$$

A being a factor depending principally on the mean motions of the sun and moon. And on Hansen's hypothesis, the *apparent* evection, or that of the centre of figure, will be

$$e \times A \times 1\cdot0001544.$$

On the same hypothesis, the excentricity derived from observation, being half the coefficient of the principal term of the equation of the centre, will be

$$e \times 1\cdot0001544,$$

and the theoretical evection computed with this excentricity will be

$$e \times 1\cdot0001544 \times A,$$

which is the same with that derived from observation. Hence

The theoretical evection will agree with that of observation, notwithstanding a separation of the centres of gravity and figure of the moon.

That Hansen overlooked this point is to be attributed to his method of determining the lunar perturbations by numerical computation from the various elements of the moon's motion, so that the manner in which the inequality depends on the elements does not appear. It is only when we determine the perturbations in algebraic form that this dependence appears.

Passing now from the evection, the next great perturbation of the moon's motion is the variation. But the value of this perturbation has not been accurately determined from observation, because, attaining its maxima and minima in the moon's octants, it is complicated with the moon's semidiameter and parallactic inequality. Even if the semidiameter is known, the two inequalities in question cannot be determined separately with precision, because their coefficients have the same sign in that part of the moon's orbit where nearly all the meridian observations are made. From this cause Airy's value of the parallactical inequality from all the Greenwich observations from 1750 to 1830 was $3''$ in error. And when, in his last investigation *, Airy rejected the observations previous to 1811, owing to some uncertainty as to what semidiameter should be employed, the result was still a second too small. It is therefore interesting to find what value of the variation will result if we substitute the known value of the parallactic inequality in Airy's equations for the determination of that element. Neglecting those unknowns which have small coefficients, these equations are, from 1806 to 1851,

1806–15	. .	$10\cdot66$W $+$	$28\cdot14$V $= +$	$17\cdot2$
1816–24	. .	$9\cdot45$ $+$	$30\cdot92$	$+$ $24\cdot9$
1825–33	. .	$9\cdot43$ $+$	$29\cdot26$	$+$ $42\cdot1$
1834–42	. .	$9\cdot29$ $+$	$27\cdot28$	$+$ $10\cdot8$
1843–51	. .	$9\cdot05$ $+$	$23\cdot36$	$+$ $7\cdot9$

$$\text{Sum} \quad . \quad . \quad 47\cdot88\text{W} + 138\cdot96\text{V} = +102\cdot9$$

In these equations $\text{W} \times 0''\cdot73$ represents the correction to the coefficient of variation, and $\text{V} \times 3''\cdot77$ that to the coefficient of parallactic inequality. We now know from recent special investigations that the latter coefficient is very near $125''\cdot50$. Airy's provisional one was $122''\cdot10$, whence

$$\text{V} = \frac{125''\cdot50 - 122''\cdot10}{3''\cdot77} = 0\cdot90.$$

The sum of the preceding equations gives

$$\text{W} = 2\cdot15 - 2\cdot90\text{V} = -0\cdot46.$$

The resulting correction to the provisional variation $(2370''\cdot3)$

* Memoirs of the Royal Astronomical Society, vol. xxix.

is therefore

$$-0 \cdot 46 \times 0'' \cdot 73 = -0'' \cdot 34,$$

Making the variation derived from observation .	2369"·96
While Hansen's theoretical value is	2369·86
And Delaunay's	2369·74

The differences are too minute to found any theory upon.

Leaving the evection and variation, the other inequalities are so minute that their product by Hansen's coefficient is altogether insensible.

Summing up the results of our inquiry, it appears that in the case of the evection the supposed discordance between theory and observation would not follow from Hansen's hypothesis, and therefore, even if it exists, cannot be attributed to that hypothesis as a cause. In the case of the variation no such discordance has been proved. In the case of the other inequalities the discordance would be insensible.

The hypothesis is therefore devoid of logical foundation.

VI. *On Extraordinary Agitations of the Sea not produced by Winds or Tides.* *By* Richard Edmonds, *Esq.**

ONE of those not infrequent agitations of the sea, which are always accompanied by earthquakes or thunderstorms, or great maxima of the thermometer, or considerable minima of the barometer—and sometimes by all these together—but which are never occasioned by winds or tides, was observed in Mount's Bay on the 6th of May, 1867, and another early on the following morning at Plymouth.

At Penzance Pier, on the first of these days, at 5 A.M., a tide-like " wave 4 to 5 feet high, without a moment's notice, swept into the harbour. A vessel in the act of moving from the new pier to the old was whirled round, and the pilot feared she would have become unmanageable. The large trawlers were swept against each other; and the sand at the entrance to the harbour was washed up, so as to colour the water for a considerable distance." The agitation continued nearly two hours; and a friend to whom I wrote for information replied that he was informed, by an eye-witness who had watched it for an hour after the first influx, that the duration of each efflux as well as of each influx was from three to five minutes. " The sky at the time was very overcast, and at 11 A.M. there was thunder with three or four flashes of lightning away to the S.E." The barometer at 9 A.M. was 29 in., the maximum of the thermometer 64°, which are

* Communicated by the Author, having been read before the Royal Geological Society of Cornwall on the 3rd of November, 1868.

respectively lower and higher than on any other day of the year up to June. The maximum of the thermometer at Plymouth this day was 74°, the minimum of the preceding night 44°, showing a range of 30°, whilst at Penzance the contemporaneous range was only 15°. The weather at Plymouth this forenoon was very foggy, and of such an unusual character that on going out of doors into the street I felt as if entering a hothouse. The above readings of the barometer and thermometer are from the registers kept at Penzance by Mr. Richards, and at Plymouth by Mr. Merrifield.

On the following morning, the 7th of May, at the Plymouth Great Western Docks, when the gatekeeper went to close the gates at high water about 7.30, he observed to his surprise an extraordinary current rushing through them into the dock, and rising to the perpendicular height of one foot above the proper level of the tide at that time. When it ceased he immediately closed the gates, and did not wait to see if it were succeeded by other such currents. The thermometer this day in Plymouth was 78°, the maximum of the year up to the 11th of June.

I have, in my last paper (read before the Society in 1865*), described a similar but much greater disturbance of the sea in Mount's Bay and Plymouth on the 14th of October, 1862, and have therein referred to those of the 31st of May and 8th of June, 1811, 5th of July and 30th of October, 1843, 23rd of May, 1847, and the 25th–26th of June and the 4th of October, 1859—which also occurred contemporaneously in those places, and which I had previously described†, together with the accompanying states of the atmosphere.

The only simultaneous disturbance of the sea of this nature in Mount's Bay and Plymouth during the last century which I can find recorded besides those on the days of the two great earthquakes of Lisbon, is that of the 28th of July, 1761, when the sea in Mount's Bay rose six feet above its proper level. Borlase, after having described it, states that there was "thunder at times all the day," and at 8 p.m. the church of Ludgvan, of which he was the rector, was struck by lightning‡.

Thus have all the recorded extraordinary agitations of the sea in Mount's Bay and Plymouth, except those on the days of the two great earthquakes of Lisbon, which they perfectly and in all respects resembled, been accompanied with thunderstorms or great maxima of the thermometer or considerable minima of the barometer. And because they are always, or almost always,

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* This paper appeared in the Philosophical Magazine for January 1866, pp. 45–52.

† See my former papers in the 'Transactions' of the Society, and my 'Land's End District,' p. 101.

‡ Phil. Trans. vol. lii. p. 507.

thus accompanied in all parts of the world, whether on sea-coasts or in lakes, distinguished authors on both sides of the Atlantic have ascribed them (when unaccompanied with *known* earth-quakes) to storms or unusually great and sudden augmentations or diminutions of the atmospheric pressure on the surface of the water. But this hypothesis is quite at variance with known facts, and so are all the other hypotheses hitherto advanced by geolo-gists, as I have shown in the printed Transactions of this Society and elsewhere.

My attention was first drawn to this subject by having wit-nessed one of these disturbances in Mount's Bay more than twenty-five years since, which I described first in a letter printed in the Literary Gazette of the 15th of July, 1843, and after-wards more fully in the paper which I read before this Society in the same year. During this long period every fact I have met with connected with the subject confirms me in the opinion that the hypothesis which I then, and in subsequent years, sub-mitted to this Society is the only one capable of reconciling all the observed facts—that hypothesis being that all these extra-ordinary agitations (of which each efflux, as well as each influx, occupies generally about five minutes, and never more than ten on our coasts) are produced by local submarine shocks of earth-quakes without any upheaving, subsidence, dislocation, or frac-ture of the submarine ground. The way, too, in which they are produced on shores or submarine ground sloping outward, I have also fully explained, viz. by the continually repeated dash-ings *seaward* of the surface of the water by the vertical and rapid vibrations of the submarine ground, until a very broad, but not high, wave is raised, and much of the shore is consequently left dry. When the vibrations have ceased, the dashed-off waters return *shoreward* to find their level, and move up and down on the shore like a pendulum until the equilibrium is restored. That a shock does generally consist of a rapid succession of countless vibrations is evident, not only from its being felt at *sea* "like the letting out of a cable," and on *land* "like a wagon rushing over a paved road," but by its very names of earthquake, *tremblement de terre*, and *seismos*.

These tide-like movements of the sea are totally unlike the agitations produced by storms. Of these latter agitations we had a memorable example on the 24th of April last at the Scilly Isles and along the southern coasts of Cornwall and Devon, when the waves rose so enormously, and their spray ascended to such an unusual height, that no one who witnessed the spectacle can ever forget it. The master of the Great Western Docks in Mill-bay, Plymouth, informed me that, as the huge waves rolled in and the vessels outside the docks mounted over them, great parts

of their keels were out of water, and he could see under them ; and at high water the waves swept over the gates into the docks, a thing never known before.   The cause of this unusual disturbance was no doubt a storm from the south-west, some hundreds of miles off in the Atlantic, which never reached us.   These huge breakers would have commanded the attention of *every one*, however unobservant, who happened to be near and in sight of them. But the tide-like currents which I have described, when occurring on an open shore would not attract the attention of *any one*, however observant, even if he were close to them, unless he watched them for some time, or unless he knew whether the ordinary tide were then coming or going, and saw that this extraordinary current was moving in an opposite direction.   This quiet tide-like motion of the water up and down an open beach was strikingly shown during one of the extraordinary agitations of the sea in 1843, by the conduct of some children, who were then amusing themselves on the beach and rocks, near the Chimney Rock in Penzance, and had been there long enough to observe that the sea was flowing in and out very differently from usual, and at intervals of about ten or fifteen minutes. Instead of running home at once and telling their parents what an extraordinary thing they had seen, they preferred making use of their knowledge for some new kind of play ; and accordingly, when they were joined on the beach by other children who had not been present long enough to make the same discovery as themselves, they told them to go out on the rocks to see something. These new comers, therefore, suspecting nothing, and observing, as they thought, that the tide was going and had left the rocks dry, and not dreaming that there was any possibility of the sea coming in again for some hours, went on the rocks as they were told by their playfellows ; and before they could return, the current surrounded them.   After waiting a few minutes in a great fright lest they should be drowned, the sea began again to ebb, and in a few minutes more they reached the beach in safety.

P.S.—These phenomena commence generally with an efflux or retirement of the sea.   And it appears from the newspapers that those accompanying the great earthquake of the 15th of August last along the western coasts of South America began with an efflux, as did also those on *the same day* in New Zealand, when " the sea rushed out and in with extraordinary violence, and in some places in the South Island great damage was done by the sea going far over the usual high-water mark."   This rushing out and in of the sea in New Zealand and on the South-American coast was in all probability occasioned in each place by local submarine shocks in the way I have above described.

But because no earthquake shock was felt on that day in California, nor in New Zealand until a day or two afterwards, the writer of the article in the 'Times' of the 3rd of November, beginning with "There are earthquakes in divers places," boldly states, without referring to any authority, that " on the 15th of August the most terrible earthquake ever known wrecked and then drowned many hundreds of miles of the South-American coast, sending a wave as far north as California, and right across the Pacific (8000 miles) to our own countrymen in New Zealand." This untenable hypothesis I have already fully disproved in my last paper above referred to, which I wrote in answer to a similar hypothesis advanced by Mr. Mallet, who in the 'Quarterly Journal of Science' for January 1864 (p. 68) states that " the *great sea-wave* of translation rolls in often hours after the shock has done its work of destruction, or portions of it may roll in upon shores that have felt no shock at all. Thus in the great earthquake at Japan, which a few years ago wrecked a Russian frigate in one of the harbours there, the great sea-wave produced in the deep sea near those islands hours afterwards reached the opposite shores of the Pacific at St. Diego and Francisco."

The writer in the 'Times' would, I think, have given a much more credible explanation of the recent agitations on the shores of the Pacific, had he said that the great earthquake of the 15th of August was not confined to America, but appears to have extended under the Pacific to New Zealand, where, although it was not perceived on dry land, its submarine shocks produced movements of the sea similar to those on the American coasts, commencing, too, like them, with a retirement of the sea.

In agreement with what I have above stated, is the following extract from the 'Times' of December 2, 1868, from its own correspondent's letter, dated Melbourne, October 13 :—" In a recent letter I referred to the great tidal wave which at various hours of the 15th of August last rolled in on different parts of Australia and New Zealand, doing some damage at Chatham Island. . . . . . Mr. Ellery of the Melbourne Observatory, the President of our Royal Society, a very scientific man, last evening in answer to a question from Professor Wilson . . . . said he had been asked to offer a few notes on the great tidal wave. . . . The time of the wave was recorded at Sydney to a few seconds. It was also pretty accurately taken at Newcastle and in New Zealand. He had reduced the time taken at those places to Melbourne mean time; and according to that reckoning the first indication of the wave was at about half-past 2 of the morning of the 15th of August, the indication being only by the self-registering

tide-gauge ; but the great wave was not observed until twenty-four minutes past 7 on the same morning. The first indication at Newcastle was at two minutes past 7 in the morning ; two hours afterwards a pretty full wave was noticed, and five hours afterwards was the greatest disturbance of all. On the coast of New Zealand the time extended from about 5 A.M. of the 15th of August to nearly an hour after noon."

All this very strikingly coincides with the agitations in the Scilly Isles, Mount's Bay, Falmouth, and Plymouth on Whit-sunday, the 23rd of May, 1847. That in Mount's Bay was noticed as early as 5 A.M., and continued all day with *varying magnitude*, the rise and fall having been from 3 to 5 feet or more perpendicularly. In the night preceding, a slight tremor of the earth was felt in Mount's Bay by two coastguardsmen while standing on the cliff between Newlyn and Mousehole ; and " a strange noise as if underground was heard " at Scilly about the time of the oscillation of the sea there. The disturbance was at its maximum in Mount's Bay at 5 P.M., and in Plymouth between 8 and 9 P.M., and did not cease until the following day, when a more fearful movement of the sea, lasting several hours, occurred in Peru in the harbour of Callao ; and " a furious submarine earthquake was felt by the captain of the American whale frigate ' Acushuett ' about sixty miles W.S.W. from the island of San Lorenzo, at 3 A.M. of the 24th"*. These extraordinary oscillations of the sea in the west of England twenty-one years ago appear, therefore, in duration, magnitude, and varying magnitude, to bear a very close resemblance to those of the present year in Australia and New Zealand.

Having already shown how these extraordinary disturbances of the sea may be produced by local submarine shocks, I will now state how they may, during their continuance, become of greater magnitude at one time than at another. This may result from a subsequent shock coinciding with one of the effluxes, and thus making that efflux considerably greater than it would otherwise have been. The following influx would therefore rise proportionally higher on the beach than did the influx which preceded it.

It thus appears that the recent extraordinary agitations of the sea in Australia and New Zealand, as well as in Peru and California, were all produced, not by any great disturbance of any kind in mid-ocean, but by local shocks of the submarine ground upon or near which, in each locality, the agitated waters had previously rested.

Plymouth, December 1868.

* British Association Report for 1850, (Sections) p. 82.

VII. *Experimental Proof that the Electric Spark is an Electro-motor.* By E. EDLUND[*].

IN former papers[†] I have proved that by the mechanical work which the voltaic current exerts in disintegrating the poles in producing the electric light, an electromotive force results which sends a current in a direction opposite to that of the principal current. As, in an electric discharge, when sparks are formed in the air there is also a disintegration of the polar surfaces, the idea readily suggested itself that the electric spark, like the voltaic arc, might possibly be an electromotor. To investigate this point more minutely, I have, in common with Dr. Lemström of Helsing-fors, made the following experiments.

A Holtz's machine, made by Ruhmkorff of Paris, was used in the investigation. The rotating glass disk had a diameter of 55 centims., and each of the jars belonging to the apparatus had a coated surface of 42 square centims. The copper galvanometer wire was 1 millim. in diameter, and was surrounded by a layer of gutta percha 2 millims. in diameter. The entire thickness of the wire, the insulating layer included, amounted to 5 millims. This wire was coiled in forty turns round a wooden frame. The aperture inside the frame, in which the needle was suspended by a cocoon-thread, was 5 centims. in length and 3 centims. high. To protect the needle from draughts, the instrument was enclosed in a glass jar.

2. If the voltaic current which, it is assumed, is produced in the electric spark, is to be investigated by the aid of the galvanometer, the spark must be connected with the galvanometer by a closed circuit. This necessary condition was satisfied, after many vain attempts, in the following manner:—

In the following figure, A B represents the rotating disk, and *a b* the two combs. An insulated copper wire *a c* is directly connected with *a*, while the insulated wire *d e* terminates in a brass knob *d* in the neighbourhood of *b*. From *c* and *e* insulated wires pass to the knobs *f* and *g*. Two other conducting-wires lead from the points *c* and *e* to the galvanometer G. At *m* a rheostat is introduced, which consists of an insulated thin German-silver wire. If the machine is charged and the disk A B put in rotation, and if moreover the resistance at *m* and the distance between the knobs *f g* and *b d* is suitably adjusted, sparks pass simulta-

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neously between *f* and *g* and between
b and *d*. If the wires are connected
in the simple manner represented by
the figure, a difficulty is met with
which at first I could not entirely
remove. The electric discharge does
not, of course, entirely pass through
the spark *f g*, but part passes through
the galvanometer G. The galvano-
meter-wires coated with gutta percha
acquire thus an electric charge which
acts electroscopically upon the mag-
netic needle and draws it on one side.
This charge was retained for some
time after the machine was at rest,
and caused a deflection. It is possible
that this charge might have been
avoided if the galvanometer had been
differently constructed, and the con-
ducting wire had been otherwise in-
sulated.   The charge in question

was ultimately quite removed, and the position of equilibrium of
the needle made quite independent of it, by directly connecting
the points *i* and *k* with each other by a German-silver wire which
could be lengthened or shortened at pleasure.   The point *k* was
moreover directly connected with the water-pipes of the house
by the wire *l*, by which it was placed in conducting communica-
tion with the earth.   Part of the electric charge passed now
through *h* instead of through the galvanometer, and the electri-
city of either one kind or the other which might have been in
excess passed to the earth through *l*.   After these wires had been
introduced, no charge was observed in the galvanometer.   In all
these experiments the jars belonging to the electrophorus ma-
chine were used.   Without these there was indeed an electrical
discharge between *b* and *d*; but it was impossible to produce
sparks between *f* and *g*, however great was the resistance at *m*.

It is well known that the electric discharge of a battery acts
upon the magnetic needle and produces a deflection which in-
creases with the quantity of electricity.   The direction in which
the deflection of the magnetic needle occurs is the same as if the
magnetic needle were surrounded by a voltaic current which went
from the positive to the negative coating.   If, then, the knobs *f*
and *g* are so far apart that no discharge ensues between them,
the magnetic needle must make a deflection when the disk is
turned.   This was always the case; and when the disk rotated
with a constant velocity, the sparks between *b* and *d* followed
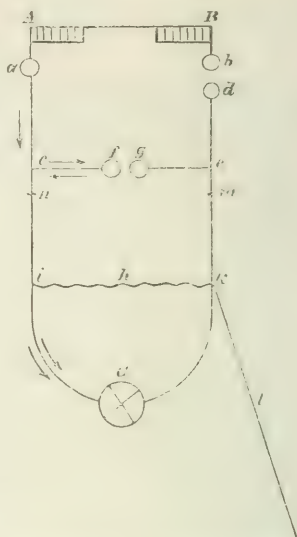
each other so closely that the equilibrium of the needle was con-
stant. The position of equilibrium could not, of course, be di-
rectly observed, because the needle constantly oscillated ; but a
knowledge of it was attained by observing the points on the scale
towards which the needle turned. If the positive electricity
passed from the comb *a* to the conduction, the deflection of
the magnetic needle was always towards the left of the scale.
When now the knobs *f* and *g* were pushed so close together that
sparks ensued between them, the deflection ought to have been less
than before, because part of the current would pass through the
conduction *c f g e*. If the spark at *f g* were only a conductor of
electricity, this must have been the case. But instead of this the
deflection was from fifteen to twenty times as much as before.
The deflection was in the same direction as if no sparks had been
formed between *f* and *g*   When the positive current starts from
a it divides at *c* ; part flows through the spark at *f g*, and another
part through the galvanometer in the same direction as indicated
by the dotted arrows. Now, if there is in the electric spark, as
there is in the voltaic arc, a galvanic current in a direction op-
posed to that of the discharge, it will traverse the conductors
in the direction indicated by the arrows ; hence there must be
deflections of the magnetic needle in the same direction as the
discharge ; and this was always the case. From this the con-
clusion must be drawn that in the electric spark there is an
electromotive force which sends a current in an opposite direction
to the discharge which the spark causes.

To test more completely the correctness of this conclusion, the
connexion between the spark-apparatus and the conducting wires
was altered as follows. This apparatus was removed from the
bridge between *c* and *e* and inserted at *n* instead. In the bridge
between *c* and *e* the rheostat was inserted which had hitherto
been at *m*. As is at once seen, the galvanic current of the spark
must now produce a deflection in the magnetic needle in a di-
rection the reverse of that of the original current ; the experi-
ments showed also that this was always the case. The magni-
tude of the deflections depends in all these cases mainly on the
distance between the knobs *f* and *g* ; the resistance in the rheo-
stat *m*, as well as that in *h*, must therefore be so great that sparks
pass between *f* and *g*, even if the distance between them is some-
what greater.

3. It seemed also interesting to ascertain whether the voltaic
spark could produce chemical decompositions. For this purpose
the conductions to the galvanometer were removed, and instead
of the rheostat wire *h* a voltameter was inserted between *i* and *k*.
The voltameter contained, as usual, dilute sulphuric acid. But
a certain proof of the decomposition of water could not thus be

obtained. Thereupon the same experiment was made with a battery of a few Bunsen's elements, after so great a resistance had been interposed in the conduction that the deflection of the galvanometer was about as great as that produced by the voltaic-spark current. But just as little with this current was there a certain proof of the decomposition of water. The currents in both cases were too feeble to produce distinct ascending gas-bubbles; the gas which is produced by so feeble a current is so small in quantity that it is partially retained by the platinum disks and partially absorbed by the liquid. It was next attempted to ascertain whether the voltaic-spark current was sufficiently strong to produce a polarization in the platinum disks. With this view a glass vessel was filled with water containing sulphuric acid, in which were placed two pieces of platinum-foil, one connected with i and the other with k. While the electrophorus disk was turned, the conducting-wires of the galvanometer were separated from i and k, but by means of a commutator could be again joined with these points as soon as the comb a was connected by means of a brass rod with the comb b and therefore all flow of electricity to the polarization-vessel ceased. If the discharge was now allowed to pass through the polarization-vessel for a given time while the knobs g and f were so far apart that no sparks ensued between them, a feeble polarization-current was always obtained when the galvanometer was connected with i and k. If, on the contrary, sparks passed between f and g while the machine was at work, after connecting the galvanometer-wires with i and k, a polarization-current was obtained which was much stronger than before. The deflection took place towards the proper side, and the current gradually diminished till it entirely ceased. If the machine was for some time in activity, the polarization-current was so powerful that the scale of the telescope was inadequate to measure the first deflection.

4. The knobs used in the spark-apparatus were of iron, brass, and tin, and all of the same magnitude, namely 17 millims. diameter. The position of equilibrium of the needle was determined by observing points on the scale where the needle turned, and taking the mean of two observations belonging to the same oscillation. For a single oscillation the needle required about eleven seconds. The electrophorus disk was turned with as uniform a velocity as possible. As the disk, when it once began to move, could not at once be brought to rest, the machine, when it was necessary to do so, was rendered inactive by connecting the two combs by a metal rod. The following are a few of the observations made:—

Series 1.

Knobs of iron at *f g*.

Experiment 1. The knobs were at a distance of 3 millims. from each other; and no formation of sparks ensued between them.

Position of equilibrium when the machine was—

A. At work.		B. At rest.
260·0		301·5
261·0		302·5
264·5	Mean .	302·0
263·0		

Mean . 262·1

For the sake of brevity, the first position of equilibrium is denoted by A, and the second by B.

Experiment 2. The knobs were just so far apart that the formation of sparks ceased.

A.		B.
297·3		299·7
297·3		299·7
297·5	Mean .	299·7

Mean . 297·4

Experiment 3. Like experiment 1.

A.		B.
257·5		300·5
258·0		300·5
259·5	Mean .	300·5

Mean . 258·3

Experiment 4. The knobs were at a distance of 1·5 millim. Formation of sparks.

A.		B.
269·0		296·0
270·8		296·0
270·5	Mean .	296·0

Mean . 270·1

Experiment 5. Distance between the knobs very small. Formation of sparks.

A.		B.
289·0		295·8
289·3		295·8
288·0	Mean .	295·8

Mean . 288·8

When there was a formation of sparks between the knobs *f* and *g*, and these were 3 millims. apart, a mean constant deflection was

obtained of 41·1 divisions. When, on the contrary, there was no discharge between f and g, the deflection only amounted to 2·3 divisions. Moreover the deflection diminished when the distance between the knobs was lessened.

Series 2.

Knobs of brass at $f g$.

Experiment 6. The knobs at a distance of 3 millims. Formation of sparks.

	A.			B.
	263·5			300·0
	264·5			299·5
	265·0	Mean	.	299·7
Mean .	264·3			

Experiment 7. The knobs removed from each other. No formation of sparks.

	A.			B.	
	297·0			299·0	
	297·0			299·3	
	297·3	Mean	.	299·2	.
Mean .	297·1				

Experiment 8. Like experiment 6.

	A.			B.
	267·0			299·5
	263·5			299·5
	262·0	Mean	.	299·5
Mean .	264·2			

The deflection obtained with brass knobs (35·4 divisions) was thus somewhat smaller than when the knobs were of iron. The deflection obtained when there was no formation of sparks between f and g was, on the contrary, as ought to be the case, the same as before.

Series 3.

Knobs of tin at $f g$.

Experiment 9. The knobs at a distance of 3 millims. Formation of sparks.

	A.			B.
	266·5			299·0
	264·0			298·5
	269·0	Mean	.	298·8
	263·3			
Mean .	265·7			

Experiment 10. Like the preceding.

A.		B.
265·5		299·0
261·0		298·6
267·5	Mean .	298·8
263·5		
Mean . 264·4		

Experiment 11. The knobs so far apart that no spark formed.

A.		B.
295·8		297·5
295·8		297·5
295·5	Mean .	297·5
Mean . 295·7		

Experiment 12. The knobs at a distance of 1·5 millim. Formation of sparks.

A.		B.
271·8		297·5
271·0		297·5
274·3	Mean .	297·5
Mean . 272·4		

Experiment 13. The knobs removed apart from each other. No formation of sparks.

A.		B.
295·0		297·0
295·0		297·0
Mean . 295·0	Mean .	297·0

When the distance between the knobs amounted to 3 millims., the mean deflection was 33·7 divisions, and therefore somewhat smaller than when brass knobs were used.

Series 4.

Both the knobs *f* and *g* were removed from the bridge between *c* and *e* and inserted at *n* instead ; *c* and *e,* on the contrary, were joined by the rheostat wire of German silver, which had previously been at *m*. The deflections thus obtained were now to the right of the scale, instead of to the left as the others had been —a deportment which can only be explained on the assumption that the spark is electromotive.

Experiment 14. Knobs of iron.

	A.		B.
	313·0		294·5
	313·0		294·5
	313·0	Mean .	294·5
	315·3		
Mean .	313·6		

The deflection amounted therefore to 19·1 divisions.

Experiment 15. Knobs of tin.

	A.		B.
	313·5		295·8
	314·0		296·0
	313·5	Mean .	295·9
Mean .	313·7		

The deflection amounted thus to 17·8 divisions.

The reason why the deflections were smaller than before was, that the resistance in the bridge only consisted of the rheostat wire m, in consequence of which the greater part of the electric discharge took this path. The distance between the knobs must therefore be small if the sparks are to jump across.

Series 5. *Polarization of platinum disks.*

A polarization-vessel was introduced between i and k instead of h. While the disk was being turned, and the current therefore passed through the liquid in the vessel, the galvanometer G was removed. After a definite time the machine was made inactive by directly connecting the combs a and b. Thereupon the galvanometer-wire was connected as rapidly as possible with the polarization-vessel, and the successive positions of equilibrium of the magnetic needle were observed.

Experiment 16. No sparks formed between f and g. The position of equilibrium of the needle before the experiment $= 219·0$. The successive positions of equilibrium after polarization were 215·0, 217·5, 217·5, 218·0. The greatest deflection, therefore, was $= 4$ divisions.

Experiment 17. Formation of sparks between f and g. The position of equilibrium of the needle before the experiment was $= 219·0$. The successive positions of equilibrium after the polarization were 192·5, 209·0, 213·0, 214·5, 215·5, 215·6, 215·5, 216·5, 217·0. The first observed position of equilibrium, after connecting the platinum disks with the galvanometer, was at 26·5 divisions.

Series 6.

To obtain a measure for the magnitude of the electromotive force, the conducting-wires of a battery were interposed between e and m. When a spark passed between f and g, the voltaic circuit was closed by the conducting spark, but was open in the other case; and the current which formed in the spark had to pass through the voltaic battery. Both currents had therefore to traverse the same circuit, and both only existed during the time in which the spark passed between f and g. The ratio between the deflections which each current produced in the galvanometer must be equal to the ratio between their electromotive forces. By means of a commutator the current of the battery might be reversed; so that it either went in the same direction as the spark-current, or in the one opposite to it. If the deflection of the galvanometer from the spark-current is x, and that from the battery-current y, we have in one case $x + y$ and in the other $x - y$. From this, x and y may be easily calculated, as well as the ratio between them, by which the electromotive force of the battery-current is known. In order that this method may be regarded as perfectly correct, the conducting-power of the spark must not vary with the direction of the battery-current. This is undoubtedly the case; for the battery-current in its passage through the spark is so enfeebled, that it can have no perceptible influence on the conducting-power. In all the following experiments the battery consisted of ten Bunsen's elements. The specific gravity of the nitric acid was $1\cdot32$. · In order that the deflections of the battery-current might be greater than in the preceding experiments, the resistances m and h were altered.

Experiment 18. The knobs f and g were of iron, at a distance of 3 millims. Formation of sparks between them. The position of rest of the needle when no current traversed the galvanometer $= 244\cdot0$.

Position of rest when the currents went in opposite directions :—

$$x - y.$$
$$179\cdot5$$
$$174\cdot5$$

Position of rest when the currents went in the same direction: ·

$$x + y.$$
$$148\cdot0$$
$$150\cdot5$$
$$\underline{151\cdot5}$$

Mean . . $150\cdot0$

Position of rest when the currents went in opposite directions:—

$$x - y.$$
$$178\cdot0$$
$$178\cdot0$$

The mean of the first two and last two observations is $= 177\cdot5$.
From this we get

$$x + y = 94\cdot0, \quad x - y = 66\cdot5, \quad x = 80\cdot3, \quad y = 13\cdot8, \quad \text{and} \quad \frac{x}{y} = 5\cdot82.$$

As the battery consisted of ten elements, the electromotive force of the spark was equal to that of $58\cdot2$ Bunsen's elements.

It must here be at the same time remarked that the deflection x is not exclusively caused by the spark-current, but part depends on the electrical discharge of the machine. To obtain at any rate a limiting value for this part the following experiments were made:—

Experiment 19. The knobs so far apart that no sparks were formed.

Position of equilibrium when the machine was at rest:—

$$231\cdot5$$
$$231\cdot3$$

Position of equilibrium when the machine was at work:—

$$225\cdot0$$
$$224\cdot7$$

The battery-current was reversed by the commutator:—

$$225\cdot0$$
$$225\cdot0$$

Position of equilibrium when the machine was at rest:—

$$231\cdot0$$
$$231\cdot0$$

Mean of the first two and last two observations $= 231\cdot2$.

From this experiment it follows that the battery-current has no influence on the magnitude of the deflection—a natural consequence of the circumstance that its circuit is open when no sparks are formed between the knobs f and g. The discharge-current, however, brings a deflection of $6\cdot3$ divisions. This deflection cannot, of course, be so great if sparks are formed between f and g; for part of the discharge passes then through the path of the sparks. That part, therefore, of the deflection x which is caused by the current forming in the spark fg cannot be less than $80\cdot3 - 6\cdot3 = 74\cdot0$ divisions. We thus find that the electromotive force of the spark cannot be less than that of $53\cdot6$ Bunsen's elements.

Experiment 20. Distance between the iron knobs 0·5 millim. Formation of sparks.

$x-y$.	$x+y$.	$x-y$.
220·5	202·0	220·5
221·0	201·8	220·0
	200·0	
	201·3	

Mean of the first two and last two observations . .   = 220·5.
Position of equilibrium when the machine was at rest = 243·1.

From this we obtain

$$x+y=41\cdot8,\ \ x-y=22\cdot6,\ \ x=32\cdot2,\ \ y=9\cdot6,\ \text{and}\ \frac{x}{y}=3\cdot35.$$

The electromotive force was thus now equal to that of 33·5 Bunsen's elements. If from $x=32\cdot2$ we subtract 6·3, it is found that the same force cannot be less than that of 27 elements.

Experiment 21. The knobs were of tin. Distance 3 millims. Formation of sparks. Position of equilibrium of the magnetic needle $=242\cdot5$.

$x-y$.	$x+y$.	$x-y$.
169·5	136·0	174·5
169·5	141·0	173·0
	135·5	
	137·5	

Mean of the first two and last two observations = 171·6.
From this we obtain

$$x+y=105\cdot0,\ \ x-y=70\cdot9,\ \ x=88\cdot0,\ \ y=17\cdot1,\ \text{and}\ \frac{x}{y}=5\cdot15.$$

The electromotive force amounts therefore to 51·5 elements. If 6·3 is subtracted from 88·0, we get as the limiting value for the same force that of 47·8 elements.

Experiment 22. The distance between the tin knobs 0·5 millim. Formation of sparks. Position of equilibrium of the magnetic needle $=241\cdot5$.

$x-y$.	$x+y$.	$x-y$.
225·0	208·0	227·0
224·0	209·0	226·5
	210·0	
Mean . .	209·0	

The mean for $x-y=225\cdot5$.
From this we obtain

$$x+y=32\cdot5,\ \ x-y=16\cdot0,\ \ x=24\cdot3,\ \ y=8\cdot3,\ \text{and}\ \frac{x}{y}=2\cdot93.$$

Thus the electromotive force $=$ that of 29·3 elements, and its limiting value $=$ that of 21·7 elements.

Experiment 23. The knobs were of brass. Distance 3 millims. Formation of sparks. Position of equilibrium of the magnetic needle $= 238 \cdot 5$.

$x-y$.	$x+y$.	$x-y$.
165·5	129·0	163·5
161·0	135·0	162·5
162·0	128·0	
	137·5	
	128·5	
Mean . .	131·6	

Mean of the first and last observations $= 162 \cdot 9$.
From this we obtain

$$x + y = 106 \cdot 9, \ x - y = 75 \cdot 6, \ x = 91 \cdot 3, \ y = 15 \cdot 7, \ \text{and} \ \frac{x}{y} = 5 \cdot 815.$$

The electromotive force in this case $=$ that of 58·15 elements, and its limiting value $=$ that of 54·14 elements.

Experiment 24. The distance of the brass knobs 0·5 millim. Formation of sparks. Position of equilibrium of the magnetic needle 238·7.

$x-y$.	$x+y$.	$x-y$.
224·0	206·5	218·0
223·5	203·0	219·0
	209·0	
Mean . .	206·2	

The mean for $x-y$ is $221 \cdot 1$.
From this we get

$$x + y = 32 \cdot 5, \ x - y = 17 \cdot 6, \ x = 25 \cdot 02, \ y = 7 \cdot 45, \ \text{and} \ \frac{x}{y} = 3 \cdot 36.$$

Consequently the electromotive force $= 33 \cdot 6$, and its limiting value that of 25·2 Bunsen's elements.

The electromotive force in the electric spark appears unexpectedly great if it is compared with the electric force in the voltaic arc. From a former determination, this, when the poles were of brass, was about equal to that of 15 elements; but in the electric spark, from what has been above said, it amounts to from 50 to 60 elements; and yet the disintegration of the poles with the luminous arc appears to be very much stronger than with the spark.

Two circumstances must here be taken into account. In the first place, the voltaic arc is continuous and the spark discontinuous. The duration of the spark, as is well known, depends on several circumstances. Under the circumstances of the present experi-

ments it cannot be more than the 0·0001 of second. The number of sparks which passed in each second did certainly not exceed 10. With this assumption we arrive at the conclusion that the passage of sparks from one pole to the other does not occupy more than 0·001 of the time during which the machine is being worked. Moreover it must be considered that each spark, as Feddersen has shown, is no continuous whole, but consists of a number of partial sparks. In order to compare the mechanical activity of the luminous arc and that of the current of sparks as regards the disintegration of the polar surfaces, we must on these assumptions first multiply that of the latter by 1000; and it is possible that in fact a still larger number must be taken. And, secondly, the metallic polar surfaces on the luminous arc have so a high temperature that they are often near their fusing-point, while with the spark they are only slightly warmer than the surrounding medium. Hence in the latter case the disintegration requires much more work than in the former. Taking into consideration this circumstance, the difference between the mechanical work in the luminous arc and that in the spark is quite different from what might at first have been supposed.

5. In the previous experiments for determining the electromotive force the distance between the knobs b and d was unchanged. To determine the influence which a change in this distance might produce, the following experiments were made:—

Experiment 25. The battery consisted this time also of ten Bunsen's elements. The distance between the knobs b and d was about 8 millims. Formation of sparks between f and g. Position of equilibrium of the magnetic needle $=231\cdot7$.

$x-y$.	$x+y$.	$x-y$.
150·0	111·5	158·0
150·5	106·0	153·0
	101·0	
	113·0	
Mean . .	107·9	

Mean for $x-y = 152\cdot8$.
From this we obtain

$$x+y=123\cdot8, \ x-y=78\cdot9, \ x=101\cdot3, \ y=22\cdot5, \text{ and } \frac{x}{y}=4\cdot50.$$

The electromotive force, therefore, is equal to that of 45 elements.

Experiment 26. Distance between the knobs b and d about 16 millims. In other respects no change. Position of equilibrium of the needle $=231\cdot0$.

$x-y.$	$x+y.$	$x-y.$
190·5	167·0	185·5
188·5	164·0	189·0
	160·0	

Mean . . 163·7

Mean for $x-y = 188\cdot4$.

From this we get

$$x+y = 67\cdot3, \quad x-y = 42\cdot6, \quad x = 54\cdot95, \quad y = 12\cdot35, \text{ and } \frac{x}{y} = 4\cdot45.$$

The electromotive force is equal to that of 44·5 elements.

From the last two experiments it follows that the electromotive force is independent of the distance between the knobs b and d. As is at once seen, y represents the conducting-power in the entire circuit; and this depends principally on the conducting-power in the sparks at f and g, and on their number. In experiment 25, y was almost twice as great as in experiment 26. Yet this is easily explained if we consider that the sparks in the first case followed each other much more rapidly than in the latter.

The subject I have here undertaken to treat is by no means completely exhausted by the experiments adduced. Several questions which are in the closest connexion with it require further investigation. It is only after these questions have been answered that it can be settled whether the electromotive force may in the future find a practical application.

PM 37(1869)

VIII. *On a Property of the Magneto-electric Current to control and render Synchronous the Rotations of the Armatures of a number of Electromagnetic Induction Machines.* By H. WILDE, *Esq.* [*]

THE discovery of the property which I am about to describe arose out of the efforts which have been made, during the last two years, to reduce the internal heat generated in an electromagnetic machine by the induction-currents set up in the electromagnet and armature by the rapid magnetization and demagnetization of the latter. This heating of the armature (as is well known) was first observed by Dr. Joule in 1843, as the result of a delicate investigation on the quantitative relation existing between ordinary mechanical power and heat[†]. In the electromagnetic machines of my invention this phenomenon unfortunately manifests itself on an alarming scale, so much so that the armature of the 10-inch machine rises in the course of a few hours to 300° F. and upwards; and were the action of the machine to be continued for any lengthened period, the insulation of the armature-coils would be in great danger of being destroyed.

[*] Communicated by the Author, having been read at a Meeting of the Literary and Philosophical Society of Manchester, December 15, 1868.

[†] Phil. Mag. S. 3. vol. xxiii. p. 264.

One method of mitigating this evil was to construct the machine of smaller dimensions, so as to afford greater facilities for the dissipation of the heat by radiation and conduction. But even in the smaller machines an inconvenient residuum of heat still remained when the machines were worked continuously for a considerable time, such as to render it desirable to adopt some means for abstracting the heat more rapidly. By means of a current of water circulating in the hollow brass segments which form part of the magnet-cylinder, Mr. Charles E. Ryder, the skilful manager at the works of Messrs. Elkington and Co., has happily succeeded in so far reducing this heating as to permit of the machines being worked for days and nights together without intermission, and without any sensible diminution of the power of the current.

The machines which have been found to be the most efficient and economical in their working are those which have armatures from $3\frac{1}{2}$ to 4 inches in diameter. The armatures are driven at about 2000 revolutions per minute; and the water, after having passed through the magnet-cylinder, is used for supplying the boilers which furnish the power for driving the machines.

I have already shown elsewhere that the current from a small magneto-electric or electromagnetic machine is sufficient to excite the great electromagnet of the 10-inch machine; and it has been further found, by my friend Mr. G. C. Lowe, that the current from one small machine is sufficient to excite simultaneously the electromagnets of several small machines. In a number of $3\frac{1}{2}$-inch machines which have been constructed under my direction for Messrs. Elkington and Co., for the electrodeposition of copper on a large scale, the currents from two $3\frac{1}{2}$-inch electromagnetic machines are made to excite the electromagnets of twenty similar-sized machines to a degree sufficient to bring out the maximum dynamic effect of each machine. The electromagnets of the two $3\frac{1}{2}$-inch exciting machines are charged by the current from a small $2\frac{1}{2}$-inch magneto-electric machine; but I have found that nearly as good a result may be obtained from the twenty machines by dispensing with the small magneto-electric machine, and employing the residual magnetism of the two $3\frac{1}{2}$-inch exciting machines in a manner similar to that described, almost simultaneously, by Mr. Farmer[*], Messrs. Varley[†], Mr. Siemens [‡], and Sir Charles Wheatstone [§].

[*] Letter to the Author, November 9, 1866, Salem, Mass. U.S., Proceedings of the Literary and Philosophical Society of Manchester, February 19, 1867.

[†] Specification filed at the Office of the Commissioner of Patents, December 24, 1866.

[‡] Specification filed at the Office of the Commissioner of Patents, January 31, 1867.

[§] Proceedings of the Royal Society, February 14, 1867.

So far I have adverted principally to the means by which a very serious defect in the practical working of the new induction machine was remedied, a defect which many of my friends, who were unacquainted with the efforts which have been made to overcome it, have considered to be fatal to the success of what seemed likely to be a useful invention.   But while the difficulty arising from the heating was now obviated, the subdivision of the materials of one large machine into a number of small ones gave rise to another defect which it was also found necessary to overcome; for although the armatures of several machines might be driven nominally at the same speed from the same driving-shaft, by means of straps, yet when the combined direct current from several commutators was required, the want of perfect synchronism in the revolution of the armatures operated to produce a diversion of the currents of some of them through the coils of others, which were at the neutral point of their revolution; and consequently the maximum useful effect of the combined currents could not be obtained.

As the high speed at which the machines were driven precluded the employment of toothed gearing, the only method which seemed at all feasible for producing the requisite synchronism of the armatures was to place a number of the machines in a straight line, and connect them together by means of a clutch fixed on the end of each armature-spindle.   The chief objection to the carrying out of this arrangement was the difficulty of providing the requisite means for preserving the synchronism of the system, when any of the intermediate machines were disabled by accident, or stopped for repairs; so that, practically, it would not have been found convenient to work more than two machines geared together in the manner described.

It was while experimenting with a pair of machines so geared together, that I first observed the phenomenon which forms the subject of this communication.   These machines were arranged for producing the electric light, with a view to their application to lighthouse illumination. The armatures were 4 inches in diameter, and each of them was coiled with a copper-wire conductor 280 feet long and $\frac{1}{8}$ of an inch in diameter.   The currents were taken from the armatures by means of copper brushes rubbing against metal rings connected respectively with the ends of the armature-coils, and were therefore in alternate directions. It has been found that alternating currents are much better adapted for the production of a constant electric light at a fixed point in space than the current which has been rectified by means of a commutator.

The clutch, by which the armatures were connected, consisted of two iron disks about 4 inches in diameter, having, in the face of

me, two iron pins which could be guided into two corresponding holes in the face of the other. These disks could be engaged or disengaged either when the machines were at rest or in motion. The relative positions of the pins and holes in the disks were such that the armatures might be engaged in reversed positions of half a revolution when required.

Each of these 4-inch machines, when making about 2000 revolutions per minute, was of itself capable of producing a very efficient electric light; and when the two armatures were clutched together in such a position that the united positive currents from both machines proceeded from one polar terminal simultaneously with the united negative currents from the other polar terminal, the sum of the currents of the two machines was obtained. On the other hand, when the armatures were clutched together in the reverse position without any change being made in the armature connexions, no current was produced outside the two machines.

These experiments, besides exhibiting the necessity of synchronous rotation, further showed that the armatures must also occupy the same relative position in the magnet-cylinders in order that the combined current from the two machines might be obtained. It now occurred to me to see to what extent the want of synchronism in the armatures would affect the magnitude of the current. The armatures were therefore unclutched and allowed to revolve independently of each other, in the same manner as when the attempt was made to take the combined *direct* current from the commutators. After the *alternating* current had been transmitted through the electric lamp for some time, I was surprised to find that there was no perceptible diminution in the amount of light produced from the carbon points, and that the current would melt very nearly the same quantity of iron wire as when the armatures were clutched together. On examining into the circumstances attending this unexpected phenomenon, I first observed that, whenever the machines were stopped, the pins and holes in the respective disks were exactly opposite each other, and that, while the armatures were revolving, the two disks could at all times be engaged and disengaged with the greatest facility. Moreover, even when, before starting the machine, the disks were set a quarter or half a revolution out of the position in which the maximum amount of current was obtained, it was found that, after the armatures had been revolving for a few moments, the disks resumed their normal position with respect to each other (as indicated by the action of the clutch) —thereby exhibiting not only the synchronous rotation of the armatures, but also that the machines contained a principle of self-adjustment to the position in which the maxi-

mum effect of the combined current was obtained. It will therefore be evident that this property of the current, to maintain the synchronism of the armatures, renders it unnecessary to employ mechanical gearing of any kind for that purpose.

Proceeding further in this investigation, I found that, in order to produce synchronous rotation, it was not at all essential that the circuit which conveyed the combined currents for producing the light should be completed, provided that the ends of the coils of each armature were connected respectively with the same metal plates which formed the polar terminals of the machines. In this case the armatures adjusted themselves to their normal positions even more readily than when the current was producing the light. The accompanying diagram will assist in explaining these observations more fully.
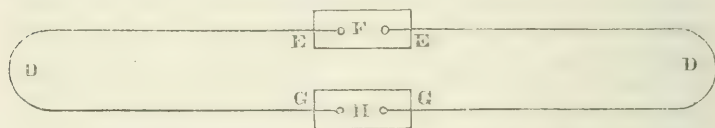
Let D and D represent the two armature-coils, which, though each 280 feet long, may virtually be represented by a single turn; E E the two outer extremities of the coils, both connected by means of the metal rings and brushes with the metal terminal plate F; G G the inner extremities of the same coils, similarly connected with the terminal plate H. The synchronous rotation of the armatures and coils D and D, as I have said, occurs either when the light is produced by the combined currents transmitted from the polar terminals F and H, or when the circuit which conveyed these combined currents is broken.

The synchronism, however, is no longer preserved when a short circuit is made between the terminals F and H by substituting a good conductor for the carbon points, or for the long piece of iron wire which was melted. Nor, again, was this synchronism preserved when contact between the metal plate H and one of the ends (G) of the coil was broken. In the latter case it was observed that, whenever contact between G and H was made and broken, a bright spark appeared at the point of disjunction so long as the rotation was not synchronous; but when the synchronism was reestablished, only a trifling residual spark was visible.

Although the synchronous rotation was preserved when the terminals F H, from which the combined current was transmitted, were disconnected from the electric lamp, yet it will be seen, from an inspection of the diagram, that a complete metallic circuit was in fact always formed between these terminals through

the coils themselves. Now, when the coils D D happen to be at the same moment in that position during their revolution in which they are producing the maximum and minimum amount of current respectively, as must often be the case where there is no synchronism, that current which is at the maximum rushes through the coil which is producing the minimum current, as is shown by the spark at the point where contact is broken between G and H. The effect of this passage of the current from one coil to the other is to accelerate or retard the rotation of the armature (according to the direction of the current) until synchronism is established.

That this influence of one coil upon the other operates in the manner described was easily shown by the following experiment :—The driving-strap of one of the armatures was removed, so that only one of the armatures should be producing a current, while the magnetism of the electromagnets of both machines was, as usual, maintained to the same degree. On placing the stationary armature with its coil in a suitable position in relation to the magnet-cylinder for producing electromagnetic rotation, and setting the other armature in motion, the stationary armature with its coil oscillated rapidly in arcs of very small amplitude, the oscillations corresponding in number with the alternations of the current. As the amplitude of the oscillations in this experiment was limited by the *vis inertiæ* of the armature, and in order that the effect of one pulsation only on the armature might be observed, contact was made and broken suddenly between the plate H and the end G of the coil by a sort of tapping motion, when the stationary armature was suddenly jerked round nearly a quarter of a revolution, sometimes in the direction in which it would have been driven by the strap, and at other times in the opposite direction, according as the alternating electrical wave which happened to be passing at the instant of making contact was positive or negative.

We have now seen, in the results obtained with the rotating and stationary armatures, a cause sufficient to account for their synchronism when revolving together,—the absence of synchronism observed when the terminals F and H were bridged over by a conductor having comparatively little or no resistance being occasioned by the controlling current traversing the short circuit established between the terminals F and H, instead of the 280 feet of resistance presented by either of the coils when approaching the neutral point of their revolution. The absence of synchronism observed when the direct current was taken from the machines by means of commutators, is caused by the direction of the current being coincident with that which they would receive by induction from the electromagnets, and consequently opposite

to that which tends to impart an accelerating or retarding impulse to the armatures.

Having obtained the full effect of the combined alternating currents from the two machines without any mechanical gearing, it yet remained to obtain the combined direct currents from the machines in the same manner. A pair of rings and a commutator were therefore fitted upon one of the armature-spindles, which was made sufficiently long for the purpose, and metallic connexion was established between the rings of each machine and the commutator on the prolongation of the armature-axis. As the commutator necessarily revolved synchronously with the two armatures, it was found that the combined alternating currents were rectified just as if they had proceeded from only one machine, and were consequently available for electrodeposition, or for any other purpose for which a direct current might be required.

Although this property of synchronous rotation has as yet been observed only in the case of several pairs and a triple combination of machines, yet there is no reason for supposing that it may not be extended to any number of machines that may be conveniently worked together from the same prime mover. It is necessary, however, to observe that as the controlling power of the current is only calculated to correct such minute deviations from synchronism as it is beyond the power of mechanical skill to prevent, the driving and driven pulleys should be respectively as nearly as possible of the same diameters, as the correction of any considerable difference in the number of the revolutions of the armatures, caused by differences in the diameters of the pulleys, must necessarily be attended by a corresponding diminution of the useful effect of the current outside the machines.

Before concluding this communication I wish to direct attention to an important property of the magneto-electric circuit which renders the commonly accepted theory, by which the generation and propagation of the electric influence in voltaic circuits is explained, inapplicable to those circuits which are entirely metallic. Reference to this property is all the more called for at the present time, as I find that a want of acquaintance with it has given rise to no small amount of misconception on the part of several eminent mathematicians and electricians who have examined my experiments on the electric condition of the earth, and the method by which I have thought proper to estimate the magnitude of powerful induction-currents*.

The intensity of a voltaic current, as represented by the mathematical theory of Ohm, is equal to the electromotive force di-

* Philosophical Magazine, August 1868.

vided by the internal resistance of the battery; and from this theory it is inferred that an electromotor, in order to overcome a great external resistance, must itself possess a correspondingly great internal resistance. A further consequence deduced from this theory is, that the maximum useful effect of a given electromotor is obtained when the external and internal resistances are equal.

Now this mode of estimating the magnitude of an electric current does not apply to the circuits on the armatures of my machines. Taking for example the results obtained from the quantity-armature of a 10-inch machine :—The dimensions of the coil of this armature may be represented by a bar of pure copper, 67 feet long, and having a sectional area of 1·6 square inch; so that the resistance which this circuit presents to the passage of a current, when compared with that of the liquids in a voltaic battery, is practically null. When the coil is in full action it will melt 15 inches of thin iron wire ·035 of an inch in diameter, or the same length of $\frac{1}{4}$-inch iron rod with equal certainty, and will electrolyze acidulated water in at least 16 voltameters in series; so that the resistance outside the circuit, whether estimated by the 15 inches of thin wire melted or by the number of electrolyzing-cells in series, is more than a hundred times as great as that of the coil in which the current is generated.

Moreover I have found that whenever a voltaic battery and a magneto-electric machine will melt an equal length of wire, the power which these electromotors have to overcome external resistance, as measured by the number of voltameters in series, is also equal. And, generally, the power of an electromotor (whether voltaic or magneto-electric) to overcome external resistance is directly proportionate to the length of wire which it will melt.

From a consideration of these results, it will be seen that one of the fundamental elements which enters into the theory of Ohm is found wanting when that theory is applied to the estimation of the magnitude of currents generated in circuits entirely metallic.

MM. Jamin and Roger, in a recent Number of the *Comptes Rendus* of the Academy of Sciences[*], have also pointed out the discrepancy here referred to in the application of Ohm's theory to magneto-electric circuits. I am, however, by no means prepared to admit the correctness of the views advanced by these physicists in their endeavours to reconcile the facts observed with established theory; besides which, other anomalies present themselves when the customary formulæ are applied to magneto-electric circuits, a consideration of which must ultimately lead

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[*] Philosophical Magazine, October 1868.

to the enunciation of laws much more general in their applica-
tion than those with which we are at present familiar.

Manchester.

P.S.—Since this paper was read, it has occurred to me that
a comparison might be attempted to be drawn between the con-
trolling power of the magneto-electric current over the rotations
of a number of armatures, and that of the voltaic current over
the oscillations of a number of pendulums. Beyond the fact
that synchronism is produced in both cases through the agency
of an electric current, there is no further resemblance between
the two actions. In the case of the armatures the synchronism
is produced by the mutual action of several rotating bodies upon
one another, or by the dominant influence of several bodies upon
one; whereas in the case of the pendulums the synchronism of
the system is produced by the influence of one body alone upon
several. Again, the synchronism of a number of pendulums is
only accomplished by the skilful adaptation of means to an end,
while the synchronous rotation of a number of armatures is a
phenomenon which exhibits itself without the exercise of any
ingenuity whatever; and, so far as I have studied this peculiar
electromechanical action, no amount of ingenuity can produce
the synchronous rotation of the armatures by means of the vol-
taic current, as magneto-electric currents and circuits seem ab-
solutely essential to the attainment of this result.

IX. *Notices respecting New Books.*

A Manual of Elementary Chemistry, Theoretical and Practical. By
GEORGE FOWNES, *F.R.S., late Professor of Practical Chemistry
in University College, London.* Tenth Edition. London, Churchill:
1868. (Pp. xxviii & 1020.)

*The Elements of Heat and of Non-metallic Chemistry. Especially
designed for Candidates for the Matriculation Pass Examination of
the University of London. By* FREDERICK GUTHRIE, *B.A.* (*Lond.*),
*Ph.D., F.R.S.E., F.C.S., late Professor of Chemistry and Physics,
Royal College, Mauritius.* London, Van Voorst: 1868. (Pp. x
& 210.)

THE different chemical manuals which appear from time to time
seem to be written from two distinct points of view—the author
desiring either to display some original mode of considering his sub-
ject, or to make an average statement of the chemical knowledge
which is accepted, in a given year, as useful to the student. Manuals
of the former class are, from their nature, not very frequently
written; while those belonging to the latter constitute the great
majority of such publications. To each kind a special merit apper-

tains ; but while the one has the narrower limit of an individual effort, it is included in the other, which hands down to the historian the general character of a time.

The present edition of Fownes's ' Manual of Chemistry ' has the curious property of blending both these distinctions. It does this by adhering to the design of the late Professor Fownes, which, if interpreted from the first edition and preface, was to lead to the principles of chemistry by an inductive ascent, and convey as complete an impression of the entire range of the science as could fairly be expected. Hence we find a special place allotted to the analytical characteristics of the elementary bodies, brief notices of laboratory operations, of higher researches, of the relations of chemistry to physics and biology. Such a plan was not then a novelty abroad ; but no students' book of this kind had, so far as we are aware, appeared in this country. The sale of nine editions in twenty-four years, and the almost universal approval of English teachers, are gratifying proofs both of the value of the original conception and of the manner in which it has been carried out at subsequent intervals.

Though succeeding editions have thus been invariably prepared upon the primitive model, the progress of science has made considerable readjustments and amendments necessary to them ; and the influence of all the great ideas which have arisen in chemistry since 1844 may be readily observed here at the appropriate epoch. But, unfortunately, in this process the modest octavo of less than six hundred pages has so far overgrown as to contain at present more than a thousand. We cannot help thinking that some part of this growth is excessive, and that means might be taken to repress it with advantage. The physical introduction, for example, is no longer justifiable, when physics is beginning to be taught (even in elementary schools) by a distinct official, or as a distinct subject from chemistry. If that were omitted, about one-eighth of the entire volume (or one hundred and twenty-five pages) would be removed, referring to subjects which, as their very able writer occasionally admits, cannot be satisfactorily treated in so small a compass. For the electricity and crystallography afterwards described, and occupying about thirty pages, the student might also be referred to other and appropriate quarters. Most of the Tables, too, at the end of the book are usually sought for in larger works, and seldom noticed in their present position. An additional reason for this curtailment is to be found in the increased length of the sections which are devoted to chemistry proper. On account of the growing attention which is now paid to inorganic chemistry, there is much fresh matter to summarize and record in that department. Still, the number of those who pursue organic research preponderates ; and it is here consequently that we notice the greatest enlargement in the size of the manual.

The best mode of treating the multitudinous detail which the science continues to produce is a point upon which chemists either doubt or disagree. The philosopher laments a dreary desert of facts fruitless and even dangerous for want of law ; the teacher bewails each serious trifle that he is compelled to read, as adding only to the

dust and effort of his pilgrimage. It is to be hoped that something may be done in the next decade to disencumber chemistry of the inconvenience and reproach of want of generalization. Meanwhile, however, a contrivance which has been frequently resorted to of late years has a certain temporary value; we allude to the statement of general formulæ for series of bodies, followed by the particular description of the constituent members. This is the method pursued in the organic part of the work now before us, and adds much to its clearness of exposition; but it is impossible to avoid feeling that there is a great deal of matter given which the average student never reads and never will be called upon to read with advantage. There is scarcely a single group of substances of whose individuals it is desirable or necessary for him to know more than a few; and an excess of information has the demerit of making chemistry positively less accessible to him. Accordingly the removal or abridgment of one or several paragraphs in the description of the various series would, in our opinion, render Fownes's manual a much more useful aid to learning. The inorganic part, though of course admitting of very little seriation, would obviously allow of considerable curtailment of a like nature.

The chapter on the General Principles of Chemical Philosophy has been very carefully rewritten, and is as compact and lucid an expression of the prevailing views as could be desired. The reader's attention is frequently called to such matters in connexion with special instances which subsequently occur; and, in the organic section more particularly, characteristic reactions of groups are pointed out with exemplary copiousness and accuracy. Such is the case under "Alcohols," "Aldehydes," "Ketones," &c. The manner in which this task is performed is one of the best criterions by which to estimate the value of a manual, and the care and talent of its writer; for here he is performing his highest duty, namely, *developing the idea of a chemical function.* We do not remember any book of the kind where that duty has been so well fulfilled as in the present instance.

Among the novelties in this edition we notice Erlenmeyer's judicious definition of *an* equivalent (a definition which did not appear too early), with the consequent division of the elements into " monogenic and polygenic." The primary classification of the elements is of course based on their " equivalence or atomicity;" but the secondary classification seems to have been made on the natural-history principle. The nomenclature employed in this manual partakes quite appropriately of both of the existing usages; so that we find such names as " sodium phosphate" (taken from the mode advocated by Harcourt and Roscoe) and " ferric chloride " (from the far preferable Berzelian nomenclature lately revived by Williamson).

Fownes's manual undoubtedly owes its success in recent times chiefly to its representative character, which has rendered it, perhaps, the most popular and useful of its class. On no other occasion has the editorial work been better executed than in the present edition, which contains a large amount both of new matter and fresh arrange-

ment. We shall not, we trust, be deemed to exceed our legitimate province when we say that, while the entire volume has received admirable supervision from both of the gentlemen to whose care it has been entrusted, most of the labour has fallen to the share of Mr. Watts, a chemist whose long experience and high literary ability have conferred on the work a completeness and finish it could not otherwise have possessed. The result of his attention is a faithful portrait of modern dogmatic chemistry. Hereafter (when dynamical theory shall have displaced or greatly modified the atomic and other statical speculations) we may look upon this picture with regret, but its truth we shall never be able to deny. The science is a prisoner in the enchanted castle of the Absolute, and still awaits some knight to rescue and deliver her.

The object of Professor Guthrie's little book is sufficiently evident from its titlepage, and does not call for a lengthy notice on our part. The writer's aim has been to produce a students' manual which shall " contain all (and but little more than all) that is required " for the chemical branch of the Matriculation Pass Examination of the University of London. We have already commented upon the association of physical with chemical subjects in the same volume, and cannot but believe that, at no distant date, the subject of Heat will be placed in its true position among the subjects selected for that examination by the Senate. That it is not so placed at present, constitutes, however, Professor Guthrie's justification for the course he has pursued. The work itself is concise, decided, and clear. The nomenclature (" chloride of silver," " carbonate of calcium," &c.) in the chemical part is open to objection as not being sufficiently modern ; and the mode of calculation under " Expansion " (§ 18) in the physical part might undoubtedly have been simplified. But these blemishes do not seriously affect the general character of the manual, which, as we have said, possesses the characteristics which are most valued in such publications.

X. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. xxxvi. p. 394.]

June 18, 1868.—Lieut.-General Sabine, President, in the Chair.

THE following communications were read :—

" An Account of certain Experiments on Aneroid Barometers, made at Kew Observatory, at the expense of the Meteorological Committee." By B. Stewart.

In judging of the value of an instrument such as an aneroid, it is not the mere extent of difference between its indications and those of

a standard barometer that ought to guide us, but it is rather the constancy of its indications under the various circumstances to which it may be subjected that determines its value.

An aneroid may differ from a standard barometer at the ordinary pressure, and to a greater extent at other pressures ; but, provided these differences can be well ascertained and remain constant, such an instrument ought to be regarded as valuable, just as much as a chronometer of known constancy, but of which the rate is wrong.

The circumstances which may be supposed to affect the indications of an aneroid may be classed under three heads, namely :—

> (1) Time.
> (2) Temperature.
> (3) Sudden variations of pressure.

(1) *Time.*—Of the influence of time I am not able to say much ; Captain Henry Toynbee has allowed me to examine the various readings of an aneroid which he carried about with him for many years in his voyages, and constantly compared with a standard barometer.

This aneroid (which I shall call No. 1) was between 4 and 5 inches in diameter, and was compensated for temperature.

In July 1860, as compared with a standard barometer, it read 0·025 in. too low. In September 1862 it read (at the same temperature) about 0·012 in. too low ; while in March 1864 (still at the same temperature) it read about 0·020 in. too low.

This instrument, which was well cared for, and which, being used chiefly on the surface of the ocean, was subjected neither to a very great nor to a very sudden change of pressure, must be allowed to have retained its character with great constancy.

This is the only definite information regarding the effect of time on these instruments which I have received.

(2) *Temperature.*—A good aneroid is generally compensated by its maker for the effects of temperature ; and the question to be investigated is, to what extent such compensations are trustworthy. I record the results (obtained at the Kew Observatory) of subjecting six aneroids, each $4\frac{1}{2}$ inches in diameter, made by two different makers, to a very considerable range of temperature.

No. of instru-ment.	Correction at				
	55° F.	72° F.	78° F.	88° F.	100° F.
2.	—·105	—·135	—·140	—·145	—·145
3.	—·055	—·090	—·095	—·095	—·100
4.	—·095	—·095	—·095	—·080	—·060
5.	—·106	—·106	—·111	—·111	—·111
6.	—·101	—·111	—·111	—·106	—·106
7.	—·061	—·061	—·061	—·061	—·031

These results are, on the whole, very satisfactory, and appear to show that a well-made compensated instrument has its indications comparatively little affected by a very considerable temperature-change.

It ought always to be borne in mind that an aneroid is not capable of being read to the same accuracy as a standard barometer, and that the $\frac{1}{100}$ of an inch is a very small quantity. These temperature experiments were made at the ordinary atmospheric pressure.

I am unable to say what effect a change of temperature would have at a diminished pressure.

(3) *Sudden changes of pressure.*—A preliminary investigation, made at the request of Mr. De La Rue, into the behaviour of an aneroid belonging to the Italian Government, seemed to show considerable error at low pressures. For the purpose of investigating the influence of sudden changes of pressure upon the indications of aneroids, I then applied to some of the best known makers of these instruments, for the loan of several, and through their courtesy in lending me a sufficient number, and for a sufficiently long time, I have been enabled to investigate this influence at some length.

In the following experiments the instruments were, to begin with, suspended vertically, at the usual atmospheric pressure. They were tapped before being read. The pressure was then lowered an inch, and the instrument allowed to remain ten minutes at this pressure before being read, after having again been well tapped.

The pressure was thus reduced an inch every time, being allowed to remain ten minutes at each stage; the instrument was always well tapped before being read, by means of an arrangement contrived for this purpose by Mr. R. Beckley. The exhaustion was carried downwards to 19 inches in the case of those instruments in which the scale was sufficiently great, and the instrument was allowed to remain an hour and a half at its lowest pressure; the air was then admitted an inch at a time, the previous arrangement as to time and tapping being followed.

TABLE I.

Correction at

No. of aneroid.	Size.	Date of trial.	30 inches. Down.	30 inches. Up.	29 inches. Down.	29 inches. Up.	28 inches. Down.	28 inches. Up.	27 inches. Down.	27 inches. Up.	26 inches. Down.	26 inches. Up.	25 inches. Down.	25 inches. Up.
8.	4½ inches.	1867. June	*		-·10	+·07	-·12	+·10	-·10	+·14	-·10	+·15	-·06	+·19
9.	4½ inches.	1867. June		+·03	-·17	+·03	-·17	+·06	-·17	+·10	-·16	+·10	-·18	+·11
8.	2nd trial.	1867. July		+·06	-·16	+·09	-·15	+·10	-·12	+·14	-·12	+·19	-·07	+·22
9.	2nd trial.	1867. July		+·07	-·12	+·07	-·10	+·10	-·07	+·13	-·07	+·17	-·07	+·15
10.	2½ inches.	1867. June		+·06	-·15	+·09	-·15	+·12	-·12	+·17	-·08	+·19	-·06	+·22
11.	2 inches.	1867. July		·:	-·20	-·05	-·21	-·02	-·22	-·03	-·22	·00	-·19	+·03
12.	2 inches.	1867. July		·00	-·19	+·02	-·19	+·06	-·18	+·05	-·13	+·12	-·10	+·14
13.	2 inches.	1867. July			-·44	-·16	-·41	-·06	-·36	·00	-·42	·00	-·35	·00
14.	4½ inches.	1867. June		-·19	-·10	+·07	-·08	+·11	-·03	+·15	-·01	+·16	-·01	+·16
15.	4½ inches.	1867. August		+·06	-·11	+·03	-·15	·00	-·15	+·01	-·14	+·03	-·12	+·03
16.	2½ inches.	1867. June		+·01	-·12	+·03	-·10	+·06	-·10	+·07	-·06	+·10	+·02	+·13
17.	2 inches.	1867. June		+·15	-·10	+·20	-·05	+·29	+·02	+·37	+·08	+·44	+·13	+·46

No. of aneroid.	Size.	Date of trial.	24 inches. Down.	24 inches. Up.	23 inches. Down.	23 inches. Up.	22 inches. Down.	22 inches. Up.	21 inches. Down.	21 inches. Up.	20 inches. Down.	20 inches. Up.	19 inches. Down.	19 inches. Up.
8.	4½ inches.	1867. June	-·07	+·20	·00	+·23	+·04	+·23	+·07	+·21	+·12	+·21	+·18	+·18
9.	4½ inches.	1867. June	-·17	+·11	-·14	+·09	-·10	+·11	-·04	+·11	+·01	+·10	+·05	+·05
8.	2nd trial.	1867. July	-·01	+·24	-·04	+·27	+·01	+·25	+·03	+·26	+·09	+·17	+·16	+·25
9.	2nd trial.	1867. July	-·08	+·15	-·08	+·14	+·06	+·12	-·03	+·16	·00	+·16	+·05	+·13
10.	2½ inches.	1867. June	-·13	+·21	-·05	+·17	+·09	+·21	-·05	+·20	+·12	+·17	+·15	+·17
11.	2 inches.	1867. July	-·16	+·06	-·15	+·08	·00	+·11	+·06	+·09	-·09	+·07	-·06	+·11
12.	2 inches.	1867. July	-·12	+·14	-·11	+·15	-·15	+·24	-·12	+·09	+·09	+·07	+·07	+·26
13.	2 inches.	1867. July	-·34	·00	-·33	·00	-·05	+·01	+·02	+·24	+·08	+·27	+·07	-·05
14.	4½ inches.	1867. June	+·01	+·13	-·02	+·04	-·29	+·01	-·26	·00	-·18	·00	-·13	
15.	4½ inches.	1867. August	-·05	+·08	+·01	+·09								
16.	2½ inches.	1867. June	+·01	+·12	+·04	+·10								
17.	2 inches.	1867. June	+·18	+·49	+·28	+·48								

* Inadvertently in these experiments a single observation in the receiver at the ordinary pressure before exhaustion was not made: but the reading at this pressure was supposed to be represented by the mean of those readings made in the open air for several days before the experiment. As, however, such readings are not strictly comparable with those made in the receiver, they have been omitted.

This Table may be better followed by considering, in the first place, the

down readings, and by supposing each aneroid right to start with (say, right at 29 inches)—that is to say, by correcting each instrument for index-error. We thus obtain :—

TABLE II.

Correction for the Down readings of the Aneroids of Table I., supposed right at 29 inches.

No. of aneroid.	Size.	Date of trial.	29 inches.	28 inches.	27 inches.	26 inches.	25 inches.	24 inches.	23 inches.	22 inches.	21 inches.	20 inches.	19 inches.
8.	4½ inches..	June 1867 ..	·00	−·02	·00	·00	+·04	+·03	+·10	+·14	+·17	+·22	+·28
9.	4½ inches..	June 1867 ..	·00	·00	·00	·00	−·01	·00	+·03	+·07	+·13	+·18	+·22
8.	2nd trial ..	July 1867 ..	·00	+·01	+·04	+·04	+·09	+·09	+·12	+·17	+·19	+·25	+·32
9.	2nd trial ..	July 1867 ..	·00	+·02	+·05	+·05	+·05	+·04	+·04	+·06	+·09	+·12	+·17
10.	2½ inches..	June 1867 ..	·00	·00	+·03	+·07	+·09	+·02	+·12	+·15	+·21	+·27	+·30
11.	2¾ inches..	July 1867 ..	·00	−·01	−·02	−·02	+·01	+·04	+·05	+·05	+·08	+·11	+·14
12.	2 inches ..	July 1867 ..	·00	·00	+·01	+·06	+·09	+·07	+·08	+·14	+·21	+·27	+·26
13.	2 inches ..	July 1867 ..	·00	+·03	+·08	+·02	+·09	+·10	+·11	+·15	+·18	+·26	+·31
14.	4½ inches..	June 1867 ..	·00	+·02	+·07	+·09	+·09	+·11	+·08				
15.	4 inches ..	August 1867 ..	·00	−·04	−·04	−·03	−·01	+·06	+·12				
16.	2¼ inches..	June 1867 ..	·00	+·02	+·02	+·06	+·10	+·13	+·16				
17.	2 inches ..	June 1867 ..	·00	+·05	+·12	+·18	+·23	+·28	+·38				

If now we separate the results of Table II. into two sets, one comprising large (4 to 4½ inch) aneroids and the other small instruments, we shall find the mean down correction for large aneroids to be as follows.

	29 in.	28 in.	27 in.	26 in.	25 in.	24 in.	23 in.	22 in.	21 in.	20 in.	19 in.
Mean correction for large aneroids, graduated to 19 in. ..	·00	·00	+·02	+·03	+·04	+·04	+·07	+·11	+·14	+·19	+·25
23 in.	·00	−·02	+·02	+·03	+·04	+·08	+·10				

In like manner we shall find for small aneroids, supposed right at 29 inches, the following mean correction :—

	29 in.	28 in.	27 in.	26 in.	25 in.	24 in.	23 in.	22 in.	21 in.	20 in.	19 in.
Mean correction for small aneroids, graduated to 19 in. ..	·00	+·01	+·02	+·03	+·07	+·07	+·09	+·12	+·17	+·23	+·25
23 in.	·00	+·03	+·07	+·12	+·16	+·20	+·27				

It will be seen that there are two instances in which the same instrument has been twice experimented on. Assuming that the mean of the two experiments represents the true correction for each of these instruments, we find :—

	29 in.	28 in.	27 in.	26 in.	25 in.	24 in.	23 in.	22 in.	21 in.	20 in.	19 in.
No. 8. Mean correction, deduced from two experiments	·00	−·01	+·02	+·02	+·06	+·06	+·11	+·15	+·18	+·23	+·30
Mean *minus* first determination ..	·00	+·01	+·02	+·02	+·02	+·03	+·01	+·01	+·01	+·01	+·02

In like manner :—

	29 in.	28 in.	27 in.	26 in.	25 in.	24 in.	23 in.	22 in.	21 in.	20 in.	19 in.
No. 9. Mean of two experiments	·00	+·01	+·02	+·03	+·02	+·02	+·03	+·06	+·11	+·15	+·20
Mean *minus* first determination ..	·00	+·01	+·02	+·02	+·03	+·02	·00	−·01	−·02	−·03	−·02

We see from these results, if aneroids, right to begin with, be subjected to a decrease of pressure similar to that to which they were subjected in these experiments :—

(1) That a well-constructed large aneroid will not go far wrong down to 24 inches ; but after that pressure its reading will be considerably lower than that of a standard barometer, so that a large positive correction will have to be applied.

(2) That small aneroids are less trustworthy than large ones, and probably cannot be trusted below 26 inches.

(3) That if previous experiments are made upon an aneroid, we are enabled by this means to obtain a table of corrections which, when applied to future observations with the same instrument, will most probably present us with a much better result than had we not verified our instrument at all, and that by this means we may use our instrument down to 19 inches with very good results.

Let us now consider the up readings of these instruments, and let us suppose that each instrument is right to begin with—that is to say, while remaining an hour and a half at its lowest reading.

These corrections and up readings are exhibited in the following Table :—

Table III.

No. of aneroid.	Correction for the up readings of the aneroids of Table I. supposed right with standard at lowest reading.											
	19 in.	20 in.	21 in.	22 in.	23 in.	24 in.	25 in.	26 in.	27 in.	28 in.	29 in.	30 in.
8.	·00	+·03	+·03	+·05	+·05	+·02	+·01	−·03	−·04	−·08	−·11	−·18
9.	·00	+·05	+·06	+·06	+·04	+·06	+·06	+·05	+·05	+·01	−·02	−·02
8.	·00	+·02	+·01	·00	+·02	−·01	−·03	−·06	−·11	−·15	−·16	−·19
9.	·00	+·03	+·03	−·01	+·01	+·02	+·02	+·04	·00	−·03	−·06	−·06
10.	·00	·00	+·03	+·04	+·03	+·07	+·05	+·02	·00	−·05	−·08	−·11
11.	·00	−·04	−·02	·00	−·03	−·05	−·08	−·11	−·14	−·13	−·16	−·14
12.	·00	+·01	−·02	−·02	−·11	−·12	−·12	−·14	−·21	−·20	−·24	−·26
13.	·00	+·05	+·05	+·06	+·05	+·05	+·05	+·05	+·05	−·01	−·11	−·14
14.	...	...	...	...	·00	+·09	+·12	+·12	+·11	+·07	+·03	+·02
15.	...	...	...	...	·00	−·01	−·06	−·06	−·08	−·09	−·06	−·08
16.	...	...	...	...	·00	+·02	+·03	·00	−·03	−·04	−·07	−·06
17.	...	...	...	...	·00	+·01	−·02	−·04	−·11	−·19	−·28	−·33

Hence we find the mean up correction for large aneroids :—

	19 in.	20 in.	21 in.	22 in.	23 in.	24 in.	25 in.	26 in.	27 in.	28 in.	29 in.	30 in.
Supposed right at 19 in.....	·00	+·03	+·03	+·03	+·03	+·02	+·01	·00	−·03	−·06	−·08	−·11
Supposed right at 23 in.....	..	..	..	..	·00	+·04	+·03	+·03	+·01	−·01	−·02	−·03

In like manner for small aneroids we have the following result :—

	19 in.	20 in.	21 in.	22 in.	23 in.	24 in.	25 in.	26 in.	27 in.	28 in.	29 in.	30 in.
Right at 19 in. ..	·00	·00	+·01	+·02	−·01	−·01	−·02	−·04	−·07	−·10	−·15	−·16
Right at 23 in. ..	..	..	..	..	·00	+·01	·00	−·02	−·07	−·11	−·12	−·19

As before, there are two instances in which the same instrument was twice tried ; assuming the mean of the two trials to represent the truth, we find—

	19 in.	20 in.	21 in.	22 in.	23 in.	24 in.	25 in.	26 in.	27 in.	28 in.	29 in.	30 in.
No. 8. Mean correction..	·00	+·02	+·02	+·02	+·03	·00	−·01	−·04	−·07	−·11	−·12	−·18
Mean *minus* first determination ..	·00	−·01	−·01	−·03	−·02	−·02	−·02	−·01	−·03	−·03	−·01	·00

In like manner—

	19 in.	20 in.	21 in.	22 in.	23 in.	24 in.	25 in.	26 in.	27 in.	28 in.	29 in.	30 in.
No. 9. Mean correction..	·00	+·04	+·04	+·03	+·03	+·04	+·04	+·04	+·02	−·01	−·04	−·04
Mean *minus* first determination ..	·00	−·01	−·02	−·03	−·01	−·02	− 02	−·01	−·03	−·02	−·02	−·02

We may learn from these results, if aneroids which have been subjected for at least one hour and a half to the lowest pressures which they register have the pressure increased by means of the gradual introduction of air into the receiver (after the manner already described) :—

(1) That a well-constructed large aneroid will not go far wrong for about 8 inches above the lowest pressure.

(2) That in this respect small aneroids are somewhat less trustworthy than large ones.

(3) That if the instrument read be previously tested and its corrections ascertained, we may consider it trustworthy (making use of these corrections) for up readings throughout a greater range than if it had not been so tested.

I come now to consider whether a rapid change of pressure affects an aneroid after the experiment has been completed.

The following Table will exhibit the results obtained in this direction :—

TABLE IV.

	1st Ex. 8.	1st Ex. 9.	2nd Ex. 8.	2nd Ex. 9.	10.	11.	12.	13.	14.	16.
Correction before experiment	−·10	−·12	−·13	−·09	−·12	−·11	−·13	−·47	−·04	−·05
Immediately after experiment	·00	+·03	+·06	+·07	+·06	−·03	·00	−·19	+·06	+·04
18 hours after experiment	−·07	−·03	+·04	+·02	..	−·10	−·07	−·34	+·01	+·01
48 hours after experiment	−·03	−·04	..	..	−·03	..	..	−·37	..	..
3 days after experiment	−·08	−·05	..	..	..	..	..	..	−·01	−·01
3 weeks after experiment	−·13	−·10	..	..	−·11	..	..	..	−·07	−·06

It thus appears that if an instrument reads correctly before it is put into the receiver it will read too low immediately afterwards, and that it may be some considerable time before it recovers its previous reading. The instrument cannot, therefore, be safely trusted for absolute determinations if it has been recently exposed to rapid changes of pressure.

The experiments hitherto recorded, in which an inch of pressure has been taken away or added every ten minutes, are perhaps analogous to ascents in a balloon, or descents from a mountain; they are not, however, precisely analogous to mountain-ascents, since a longer time than 10 minutes is usually taken to produce a change of pressure equal to 1 inch.

At the suggestion of Mr. Charles Brooke, a couple of aneroids were tested in April 1868, with the view of rendering the experiment more analogous to a mountain-ascent.

The pressure was reduced by half an inch at a time and at intervals of 30 minutes, the aneroids being well tapped.

The following corrections were obtained for down readings (instruments supposed right at 30 inches).

TABLE V.

At	No. 8.	No. 9.	At	No. 8.	No. 9.
inches.			inches.		
30	·00	·00	23·5	+·08	−·02
29·5	·00	− ·03	23	+·11	−·03
29	·00	−·04	22·5	+·12	−·01
28·5	·00	−·03	22	+·14	·00
28	·00	−·03	21·5	+·16	+·02
27·5	·00	−·02	21	+·17	+·04
27	....	....	20·5	+·20	+·06
26·5	·00	−·02	20	+·22	+·07
26	+·01	−·02	19·5	+·25	+·09
25·5	+·04	−·02	19	+·27	+·11
25	+·05	−·04			
24·5	+·06	−·02			
24	+·05	−·01			

These results, when compared with the previous determinations for these same instruments, would seem to show that a somewhat better result is obtained when the exhaustion is carried on more slowly, and hence that the corrections depend, to a considerable extent, on the nature of the treatment received. No. 8 seems to be more constant under different treatment than No. 9.

From all these experiments we may perhaps conclude as follows:—

(1) A good aneroid of large size may be corrected for temperature, by an optician, so that the residual correction shall be very small.

(2 α) If an aneroid correct to commence with be used for a balloon- or mountain-ascent, it will be tolerably correct for a decrease of about 6 inches of pressure.

(2 β) A large aneroid is more likely to be correct than a small one.

(2 γ) The range of correctness of an instrument used for mountain-ascents may be increased by a previous verification, a table of corrections being thus obtained.

(3 α) If an aneroid have remained some time at the top of a mountain, and be supposed correct to start with, then it will give good results for about 8 inches of increase of pressure.

(3 β) A large aneroid is more likely to be correct than a small one.

(3 γ) If the aneroid has been previously verified, it is likely to give a better result.

(4) After being subjected to sudden changes of pressure, the zero of an aneroid gradually changes; so that under such circumstances it ought only to be used as a differential and not as an absolute instrument—that is to say, used to determine the distance ascended, making it correct to begin with, or to ascertain the distance descended, making it correct to begin with—it being understood that the instrument ought to be quiescent for some time before the change of pressure is made.

Before concluding I ought to mention that most of the experiments herein described were undertaken and executed in a very careful manner by Mr. T. W. Baker.

"On an Easy Method of measuring approximately the Intensity of Total Daylight." By Roger J. Wright, Esq.

An easy method by which the amount of light may be at any time measured and registered appears to be still wanting. I would suggest the following plan, by which I believe the desired object may be attained.

A B is a rod of solid metal, terminated by a heavy base, which keeps the rod in a perpendicular position. C D is a hollow tube, blackened inside, of such a diameter as exactly to fit and slide over A B. The extremity, B, of the rod A B is painted of a snowy white, with a jet-black spot in the centre, as shown in the figure. On A B is marked the scale, beginning with zero at A. The tube is pushed over the rod till the extremity C coincides with the zero line at A.

The method of using this instrument is as follows:—Draw the tube gently up the rod, at the same time looking steadily at the black spot before mentioned. It will be found, as the tube ascends,

that the black spot will gradually disappear, and ultimately vanish in the gloom ; it will also be found that on *different* days, and *differ-*
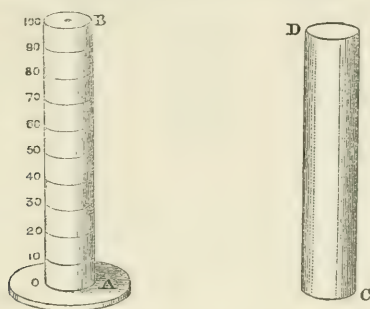
ent hours of the *same day*, the *point at which the black spot vanishes* will *vary with the intensity* of the light. This point is read off on the graduated scale, and thus we are enabled to measure the intensity of the light at any required time. In taking an observation, it would be well to state whether that portion of sky round the zenith from which the cone of rays proceeds be clear or cloudy.

It will be seen that the result obtained by this method is not *scientifically* correct, as it will be affected by the eyesight of the person who makes the observation, but only in a slight degree. The method of measuring light, as just described, has been known to me for upwards of three years. The hope that I should some day be enabled to make the instrument *scientifically* correct has hitherto prevented me from making it public. As I understand that it is highly desirable to have some means of estimating the changes in the light which will occur during the total eclipse of the sun in August next, I no longer feel justified in keeping in the background an instrument which may *possibly* be of some slight assistance.

XI. *Intelligence and Miscellaneous Articles.*

ON THE DISPERSIVE POWER OF GASES AND VAPOURS.
BY M. CROULLEBOIS.

THE measurement of the dispersive power of gases, which has long been obscurely foreseen, has not yet been supplied by any physicist by the aid of a convenient and accurate experimental method. The illustrious mathematician Cauchy, in the month of August 1836, even sent to the Academy a memoir which concluded, as a necessary consequence of his able theory of light, that this dispersion had no existence. Arago announced that in this there was an error of fact, and he promised to publish a memoir on this subject containing numerous delicate measurements ; but this memoir never appeared, and it has not been met with in his papers. We are lost in conjectures as to the method employed by this illustrious physicist to measure the dispersion of colours in elastic fluids. Doubtless

Arago had no absolute confidence in the measurements obtained by his then method, and sought a more delicate process for the observation of such delicate phenomena than the one he had used. This opinion gains in plausibility when we consider that it was by a subsequent discovery that he became possessed of a method of observation (that of interference) which has never been equalled in delicacy. I have been able to apply this process to the measuring the dispersive power of gases by two different methods. In both cases the interference-fringes were produced by means of M. Billet's semilenses.

First method.—When the ordinary apparatus are used for producing fringes, the experimenter finds that the determination of the dispersion is impossible if, working with white light, a central zone is discriminated, in reference to which everything is symmetrical, and which can be sighted. The number of fringes becomes considerable when a decidedly homogeneous light is used; a central region is no longer distinguished; the fringes of the system resemble each other; every point of comparison has disappeared, and we no longer know which of the fringes to stop at on the cross wires of the telescope. To measure dispersion it is necessary, therefore, to obtain with homogeneous light a system of fringes limited in number—a condition which has been realized by very simply modifying the semilenses.

To reduce the number of fringes, we cover by any suitable method (by a small plate or by the removal of the interposed tubes) the interior edges of the semilenses. (A lens which, cut, has undergone a loss of matter produces this result in a third manner.) Then the part which is in common, instead of extending indefinitely, may become defined, and the region which succeeds the plane of the maximum of the fringes may terminate in only giving one fringe, as takes place in the region preceding this plane of the maximum. But to observe these fringes the lens alone cannot be employed; the screen of ground glass must be used, through which they are viewed. This was Fresnel's old method. To compensate the retardation, I used M. Billet's compensator, graduated for each colour by a very simple method, which consists in determining for each of them the zero of the instrument, which is variable.

Two differential tubes containing dry gases were placed in front of the semilenses, behind a collimating lens which had the slit for focus.

In a special memoir I shall publish the numerous precautions to be taken in the arrangement of the apparatus. In the case of air, the two tubes were filled with dry air under the same pressure, and the air in one of the tubes was gradually rarefied. It was merely necessary to measure (1) the pressures H and H′ of the gases, (2) the temperature, (3) the graduation of the compensator.

For one of the colours investigated the index of the gas was given by the formula

$$N_v = \sqrt{1 + \frac{760(1 + \alpha t)}{H - H_1} \frac{2m\lambda}{E}}. \quad . \quad . \quad . \quad (1)$$

E is the common length of the tubes. N may be known for every temperature and under every pressure. For other gases than air one of the tubes was filled with dry air, the other with the gas to be investigated; the index of this latter was given by the formula

$$N'_0 = \sqrt{1 + (N''_0 - 1) + \frac{2m\lambda}{E} \left\{ N_0^2 - 1 + \frac{760}{H}(1 + \alpha t). \right\}}$$

We must know t, H, m; m is positive or negative according as the gas to be investigated is more or less refracting than air.

To have a colour of a known wave-length, a pure spectrum was produced, and one of the three lines C, E, G was made to fall on the slit which illuminated the apparatus.

Second method.—The idea of this method was suggested by the consideration of the formula (1).

In fact, let us conceive an arrangement of apparatus by which we can measure for each value of λ the number of fringes removed corresponding to the difference H—H' of the pressures in the two tubes.

The difference of pressure is measured by a kind of voluminometer; and while the mercury flows out as the elastic force of the gas in one of the tubes diminishes, the experimenter counts the number of fringes removed. In this method, singularly enough, the compensator has but a secondary part, and most frequently its use may even be suppressed.

These two methods, which were intended to control each other, have given the same results. It will moreover be understood that the first method admits of numerous means of verification.

Vapours.—These two methods of experimentation may be applied to vapours. I had intended to apply these methods to all those whose elastic forces are furnished by M. Regnault's Tables. But time and sunlight having failed me, my investigations in this direction could only be extended to bisulphide of carbon, to ether, and chloride of ethyle; and as the results obtained with these two latter compounds seem to leave something to be desired, I shall not adduce them here.

Gases.

1. *Air.*—Index of white light : 1·00 /2944.

    Line C .... $N_C = 1·0002575$.
    Line E .... $N_E = 1·0003042$.
    Line G .... $N_G = 1·0003157$.

The dispersion is therefore

    $N_G - N_E = 0·0000115$; $N_G - N_C = 0·0000582$.

2. *Carbonic Acid.*—Index of white light : 1·000449.

    Line C..... .... $N_C = 1·000395$.
    Line E .... $N_E = 1·000456$.
    Line G................. $N_G = 1·000456$.

The dispersion therefore is

$$N_G - N_E = 0 \cdot 000014 \; ; \; N_G - N_C = 0 \cdot 000115.$$

3. *Oxygen.*—Mean index : $1 \cdot 0002719$.

$N_C = 1 \cdot 000255 \; ; \; N_E = 3 \cdot 000294 \; ; \; N_G = 1 \cdot 000300.$

4. *Hydrogen.*—Mean index : $1 \cdot 000138.$

$N_C = 1 \cdot 000129 \; ; \; N_E = 1 \cdot 000140 \; ; \; N_G = 1 \cdot 000153.$

5. *Nitrogen.*—Mean index : $1 \cdot 0003019.$

$N_C = 1 \cdot 000258 \; ; \; N_E = 1 \cdot 000302 \; ; \; N_G = 1 \cdot 000321.$

6. *Chlorine.*—Mean index : $1 \cdot 000774.$

$N_C = 1 \cdot 000699 \; ; \; N_E = 1 \cdot 000792 \; ; \; N_G = 1 \cdot 000840.$

7. *Cyanogen.*—Mean index : $1 \cdot 000829$[*].

$N_C = 1 \cdot 000804 \; ; \; N_E = 1 \cdot 000834 \; ; \; N_G = 1 \cdot 000895.$

8. *Sulphuretted Hydrogen.*—Mean index : $1 \cdot 000639.$

$N_C = 1 \cdot 000599 \; ; \; N_E = 1 \cdot 000647 \; ; \; N_G = 1 \cdot 000691.$

9. *Ammonia.*—Mean index : $1 \cdot 000390.$

$N_C = 1 \cdot 000374 \; ; \; N_E = 1 \cdot 000399 \; ; \; N_G = 1 \cdot 000444.$

10. *Carbonic Oxide.*—Mean index : $1 \cdot 000344.$

$N_C = 1 \cdot 000301 \; ; \; N_E = 1 \cdot 000350 \; ; \; N_G = 1 \cdot 000391.$

11. *Olefiant Gas.*—Mean index : $1 \cdot 000669.$

$N_C = 1 \cdot 000652 \; ; \; N_E = 1 \cdot 000694 \; ; \; N_G = 1 \cdot 000702.$

12. *Marsh-gas.*—Mean index : $1 \cdot 000449.$

$N_C = 1 \cdot 000412 \; ; \; N_E = 1 \cdot 000471 \; ; \; N_G = 1 \cdot 000502.$

Vapours.

Bisulphide of Carbon.—Mean index : $1 \cdot 001592.$

$N_C = 1 \cdot 001489 \; ; \; N_E = 1 \cdot 001609 \; ; \; N_G = 1 \cdot 001795.$

The dispersion is

$$N_G - N_C = 0 \cdot 000306 \; ; \; N_G - N_E = 0 \cdot 000186.$$

It will be remembered that M. Le Roux has recently found that through the vapour of iodine the spectrum is reversed. I propose to ascertain whether the dichroism noticed by M. Le Roux is met with in the vapour of bromine ; but this investigation presents difficulties, and requires a knowledge of physical constants which have not yet been determined.

The subject of these researches, which tends to fill up a void in science, was suggested to me in the laboratory of investigation in the Sorbonne by Professor Jamin. I have been fortunate enough to be able to execute them in the physical laboratory of the Faculty of Sciences at Dijon, and under the eyes of Professor Billet, who has aided me by his advice, which is so valuable to all who are engaged in optical researches.— *Comptes Rendus*, October 5, 1868.

* We find for this gas an appreciable difference between Dulong's number and that which we have obtained.

ON THE GENERATION OF OZONE IN OXYGEN AND IN AIR UNDER
THE INFLUENCE OF THE CONDENSED ELECTRICAL SPARK. BY
MM. L'HOTE AND ST.-EDME.

Some time ago Mr. Ladd devised an electrical condenser which he proposed as capable of producing large quantities of ozone by elec- trifying oxygen. This apparatus consists of a wooden box 14 by 7 inches by $1\frac{1}{2}$ inch, inside which are six glass plates coated inside with tinfoil, arranged so that the spark furnished by an induction coil whose poles are connected with the two extreme plates sweeps by condensation all the intermediate spaces. It is known that oxygen which traverses Ladd's apparatus at the moment of discharge ac- quires a powerful odour of ozone; hence the inventor has proposed the use of the apparatus for injecting electrified oxygen into the ventilation passages of large buildings—amphitheatres, hospitals, theatres, &c.

The interest attaching to such a producer of ozone has increased since General Morin called attention to the utility which the addition of suitable quantities of ozone in ventilation might have as a de- stroyer of miasmata.

MM. Becquerel and Fremy have determined the proportion of ozone which the electric spark produces when it strikes directly be- tween two platinum wires fused in a glass tube full of pure oxygen. It is also known that under these circumstances the air becomes charged with nitrous products; but we are quite ignorant of the extent to which a spark from a powerful induction-coil, striking in a cascade condenser like Ladd's apparatus, modifies the gas in con- tact with its multifold surfaces.

We began by arranging Ladd's apparatus so that it could rigo- rously retain the pressure of the gas to be investigated, and then we estimated the proportion of ozone formed in equal volumes of oxygen and air circulating with the same velocities in the condensing-appa- ratus, and under the influence of a spark of the same force (20 to 25 centims. in air).

It has been established that the modified gas (oxygen or air) does not attack tin. Solution of iodide of potassium is immediately attacked in both cases; silver is oxidized. It is difficult to ob- serve any difference between the odours of oxygen and of air.

The measurements were made in the following manner:—The gas emerging from the condenser passed through a Will's apparatus containing a pipette of sulphuric acid in which was 0·061 grm. of monohydrated acid, to which was added a cubic centimetre of solution of neutral iodide of potassium containing 0·1 gr. of iodide. The quan- tity of ozone was determined alkalimetrically from the proportion of potash formed. The following are the results obtained :—

(1) The oxygen which passes into the condenser is pure and dry. Six estimations gave for 10 litres of gas the proportions of ozone—

milligrammes.	miliigrammes.
28·7	26·3
29·0	16·8
26·9	15·9

In other experiments the odorant gas was collected, on emerging from the condenser, in a small gasometer containing distilled water.

For 10 litres of odorant gas the proportions of ozone found were—

milligrammes.
36·00
3·90
9·00

(2) The same experiments made in air prove *that no nitrous compounds are formed in the atmosphere which emerges from* Ladd's *condenser*.

The proportion of ozone formed is less than with pure oxygen. For 10 litres the numbers found are—

milligrammes.
3·8
3·7
2·7

The successive diminution in the numbers quoted is due to the decrease in the intensity of the electrical source, since the results of experiments made near each other are almost identical. In a subsequent research we shall give the relation which may exist between the degree of ozonization of the atmosphere and the length of the spark. We can even now say that the discharge of a coil far smaller than that used for the above experiments (producing a spark of 3 centims. instead of 20) communicates almost as strong an odour to the gas.

This point is the more interesting to clear up, since experiments prove that this condenser is a continuous source of ozone for air traversing it; and since this air does not become charged with any trace of nitrous compounds, the detrimental influence of which on the organs of respiration is well known, this ozonized atmosphere, therefore, would not be at all injurious if it were introduced by ventilation.

We propose to decide by other experiments how far the difference of chemical activity may extend which at present seems to exist between the direct spark and that of condensation.—*Comptes Rendus,* September 21, 1868.

NEW METHOD OF ESTIMATING MINUTE TRACES OF METALS, SPE-
CIALLY DESIGNED FOR WATER-ANALYSIS. BY J. ALFRED
WANKLYN, PROFESSOR OF CHEMISTRY IN THE LONDON INSTI-
TUTION, AND ERNEST THEOPRON CHAPMAN.

In the concluding chapter of our short treatise on water-analysis, we made mention of the possibility of the presence of very minute quantities of metals in drinking-water exercising a considerable influence on the health of the community. A method of detecting and measuring these minute traces of metals was wanted. We are now able to supply the want.

Availing ourselves of the circumstance that sulphuretted hydrogen does not precipitate, but simply colours a very dilute solution of certain metals (the depth of coloration following the quantity of metal present in the solution), we have devised and practised a method of titration which is for certain metals much what the Nessler-titration is for ammonia. In this way we have measured the amount of lead in a specimen of Manchester water.

The practical details of the process, together with examples, will be published on a future occasion.

XII. *Remarks on Affinity. By* M. Dumas[*].

WHAT is the force which causes simple substances to unite with other simple substances to form compounds—acids with bases to form salts, quicklime with water to form slaked lime, carbon to burn in air, iron to become covered with rust?

This force we do not know. We are merely aware that it is only exerted when bodies are in apparent contact, that it becomes inappreciable when the distance of the bodies is appreciable, that, although the mass of bodies may come into play in the phenomena which it produces, their nature exerts the preponderating action. We designate it as *affinity*.

I do not propose to retrace here the history of affinity since the first appearance of this word in the doctrines of chemistry, now more than two centuries ago. I have explained elsewhere the successive interpretations which have been given to it by Barchusen, who first used it, Boerhaave, who fixed the meaning, Geoffroy, who thought he had discovered the laws, and Berthollet, who did really formulate them for a great number of phenomena.

I should even not have allowed myself to place before the Academy this fragment borrowed from the exposition of the last researches of French chemists, if, in order to show their import, I had not been led to place them in parallel with the principles established by Newton at the close of the long researches to which he devoted himself to account for the nature of chemical reactions.

But having, for my own part, been led to pay homage to the power and accuracy of his chemical knowledge, it seemed to me that at the moment in which they have been brought to the attention of scientific men by our illustrious dean, when he communicated to the Academy his very interesting philosophical studies, I might anticipate by a few weeks a publication in which they play an important part.

Newton, it is known, had made numerous chemical experiments which have been lost. The conclusions which he had drawn from them have been summed up by himself. They served as basis for the chemical doctrine of Bergmann and for that of Buffon, who, by a premature use of Newton's principles, have not a little contributed to prevent chemists from according to them the respect they merit. Thus the name of Newton has disappeared from treatises on chemistry; and I think, with M. Chevreul as well as M. Trouessart, that it ought to be restored, as he was the first to comprehend the nature of affinity.

Lavoisier, contemporary with Buffon, but more reserved with regard to a subject with whose difficulties he was better acquainted, never expressed himself definitely on the subject of affinity. He even considered this branch of science too high for the reach of the chemists of his time; and he recommended them, before busying themselves with it, to settle on a solid basis the elements of chemistry, in the same manner, says he, that we fix with certainty the principles of elementary geometry before approaching the difficulties of the higher geometry.

Lavoisier, therefore, postponing the investigation of the force which produces chemical phenomena, had concentrated his attention on the part which ponderable matter plays. He had doubtless considered the heat disengaged or absorbed in the reactions of bodies a fundamental phenomenon, the measurement of which was as necessary for their explanation as the determination of the weights of the substances employed and of the substances obtained; but we do not see that he regarded this heat as an expression of the chemical force.

Ponderable matter, heat, molecular attraction are the three terms to which Lavoisier had recourse, and with which he contented himself for the explanation of chemical phenomena. He measured most exactly and delicately matter and heat in their displacements. He left attraction on one side as a notion inaccessible to experiment, and as only giving rise in his time to useless hypotheses.

Lavoisier, then, had assumed chemical attraction, affinity, but had not endeavoured to explain it. In that respect he agreed with Newton. This great man, almost a century before, enunciated in the following terms the result of his labours and his

reflections on chemical phenomena, showing by the accuracy of his details and the depth of his views that the humble practical investigations of the laboratory were as familiar to him as the most elevated conceptions of celestial mechanics* :—

" Have not the small particles of bodies certain powers, virtues or forces by which they act at a distance not only upon the rays of light for reflecting, refracting, and inflecting them, but also upon one another for producing a great part of the phenomena of nature ? For it is well known that bodies act upon one another by the attractions of gravity, magnetism, and electricity ; and these instances show the tenour and course of nature, and make it not improbable but that there may be more attractive powers than these. For Nature is very consonant and conformable to herself. How these attractions may be performed I do not here consider.

" What I call attraction may be performed by impulse, or by some other means unknown to me. I use that word here to signify in general any force by which bodies tend towards one another, whatsoever be the cause. For we must learn from the phenomena of Nature what bodies attract one another and what are the laws and properties of the attraction, before we inquire the cause by which the attraction is performed.

" The attractions of gravity, magnetism, and electricity reach to very sensible distances, and so have been observed by vulgar eyes ; and there may be others which reach to so small distances as hitherto escaped observation ; and perhaps electrical attraction may reach to such small distances even without being excited by friction."

Newton explains by this attraction the property which certain salts possess of absorbing water from the air, and the difficulty of separating this water from them by heat ; he also thus explains the absorption of aqueous vapour by sulphuric acid, and the heat developed by mixing this acid with water :—

" When spirit of vitriol poured upon common salt or saltpetre makes an ebullition with the salt and unites with it, and in distillation the spirit of the common salt or saltpetre comes over much easier than it would do before, and the acid part of the vitriol stays behind, does not this argue that the fixed alkali of the salt attracts the acid spirit of the vitriol more strongly than its own spirit, and not being able to hold them both, lets go its own ?

* [As M. Dumas does not mention whence he has taken Newton's statements, the passages in the text are taken from the ' Optics,' Book III. (vol. iv. p. 242 *et seq.* of Horsley's edition of Newton). In some cases M. Dumas appears to have summed up in his own language Newton's views. Where it could be done without undue length the corresponding original passage has been given.—Eds.]

" When salt of tartar *per diliquium* (potash) being poured into the solution of any metal precipitates the metal and makes it fall down to the bottom of the liquor in the shape of mud, does not this argue that the acid particles are attracted more strongly by the salt of tartar than by the metal, and by the stronger attraction go from the metal to the salt of tartar ? And so when a solution of iron in *aqua fortis* dissolves the *lapis calaminaris* and lets go the iron, or a solution of copper dissolves iron immersed in it and lets go the copper, or a solution of silver dissolves copper and lets go silver, or a solution of mercury in aquafortis being poured upon iron, copper, tin, or lead dissolves the metal and lets go the mercury, does not this argue that the acid particles of the *aqua fortis* are attracted more strongly by the *lapis calaminaris* than by iron, and more strongly by iron than copper, and more strongly by copper than by silver, and more strongly by iron, copper, tin, or lead than by mercury?"

" . . . . And when metals corroded with a little acid turn into rust, which is an earth tasteless and indissolvable in water, and this earth imbibed with more acid becomes a metallic salt, and when some stones, as spar of lead, dissolved in proper menstruums become salts, do not these things show that salts are dry earth and watery acid united by attraction, and that the earth will not become a salt without so much acid as makes it dissolvable in water."

I think that no chemist contemporary with Newton had such just and sound notions of chemistry as are summed up in these lines. It is doubtful whether any one at that time understood their force and import.

We may then regard the following considerations of Newton not as vain hypotheses, but as the fruit of a very advanced experience, of long and substantial studies :—

" Now the small particles of matter may cohere by the strongest attractions and compose bigger particles of weaker virtue ; and many of these may cohere and form bigger particles whose virtue is still weaker ; and so on for divers successions, until the progression end in the biggest particles, on which the operations in chemistry and the colours of natural bodies depend, and which by adhering compose bodies of a sensible magnitude.

" If the body is compact and bends or yields to pression without any sliding of its parts, it is hard and elastic, returning to its figure with a force rising from the mutual attraction of its parts. If the parts slide upon one another, the body is malleable or soft ; if they slide easily and are of a fit size to be agitated by heat, and the heat is big enough to keep them in agitation, the body is fluid ; and if it be apt to stick to things it is humid ; and the drops of every fluid affect a round figure by the mutual attrac-

tion of their parts, as the globe of the earth and sea affects a round figure by the mutual attraction of its parts by gravity.

" Since metals dissolved in acids attract but a small quantity of the acid, their attractive force can reach but to a small distance from them. And as in algebra, where affirmative quantities vanish and cease there negative ones begin, so in mechanics, where attraction ceases, there a repulsive virtue ought to succeed. And thus Nature will be very conformable to herself and very simple, performing all the great motions of the heavenly bodies by the attraction of gravitation which intercedes these bodies, and almost all the small ones of their particles by some other attractive and repelling power which intercedes the particles.

" There are in nature agents capable of uniting the particles of bodies, and it is the province of experimental philosophy to discover these agents."

Newton proceeds:—"All these things being considered, it seems probable to me that God in the beginning formed matter in solid, massy, hard, impenetrable, moveable particles, of such sizes and figures and with such other properties and in such proportion to space as most conduced to the end for which he formed them ; and that these primitive particles being solids, are incomparably harder than any porous bodies compounded of them, even so very hard as never to wear or break in pieces."

In the same way that it would be difficult to define molecular attraction (to which chemical affinity is referred) better than Newton did, in like manner the definition which he gives of atoms would even now be the best introduction to the statement of the ideas which it is possible to form of the atoms of modern chemistry, which are identical with the particles which he calls *primigenal.* Chemists of the present time elude, it is true, the difficulty by leaving in vagueness whatever concerns the nature of affinity or that of atoms. They thus obey their praiseworthy habits of mind, preferring to pass over in silence subjects on which certainty cannot be obtained. This reserve, however, is not without inconvenience ; for those commencing the study of chemistry naturally attempt to supply the silence of their masters on these subjects, the only ones which the beginner can attack in the absence of laboratories, and when he is not led to fix all his attention on the details of experiments and the management of apparatus. It is unnecessary to add that they go astray, and that one of the principal obstacles to the diffusion of sound principles in chemistry arises probably from the ignorance in which the beginner is left as to the nature of the forces at work, and on that of the atoms it is concerned with.

" It seems to me further," adds Newton, " that these particles have not only a *vis inertiæ* accompanied with such passive laws

of motion as naturally result from that force, but also that they are moved by certain active principles, such as is that of gravity, and that which causes fermentation and the cohesion of bodies."

I will conclude these quotations by a few lines in which Newton states the true philosophy of science :—

" To tell us that every species of things is endowed with an occult specific quality, by which it acts and produces manifest effects, is to tell us nothing ; but to derive two or three general principles of motion from phenomena, and afterwards to tell us how the properties and actions of all corporeal things follow from those manifest principles, would be a great step in philosophy, though the causes of those things were not yet discovered ; and therefore I scruple not to propose the principles of motion above mentioned, they being of very general extent, and leave their causes to be found out."

Without solving the question propounded by Newton, Berthollet subsequently discovered one at any rate of the general principles of motion, the application of which to the fundamental reactions of salts upon each other, of acids and of bases on salts, constitutes what are known as *Berthollet's laws*.

If, for instance, we mix nitrate of lime and sulphate of soda, both in aqueous solution, sulphate of lime is deposited and nitrate of soda remains in solution.

Berthollet justly ascribes the exchange of the base and of the acid which has taken place, not to more energetic affinities, but to the deficient solubility of sulphate of lime. He shows that, in general, when two saline solutions are mixed, and one of the four salts capable of being formed is insoluble, this one is formed, deposited, and thus determines the production of the corresponding complementary salt.

Berthollet assigns the greater cohesion of the insoluble salt as the cause which determines its formation ; but if we endeavour to define by what signs he ascertains whether the cohesion of a salt is greater or less, we are forced to accept solubility and insolubility themselves as the only indications of the weakness or the intensity of the cohesion. Thus, in the statement of Berthollet's laws, we have long been content to say that, in the mixture of two saline solutions, if the possible insoluble salt is formed and is deposited it is because it is insoluble.

I have shown, however, that Newton with wonderful precision had indicated the greater or less force of union of the parts as one of the determining causes of fluidity or of fixity ; for what Berthollet calls cohesion consists really in a diminution of volume, in an increase of density, as my investigations on atomic volumes prove.

If, for instance, we compare magnesia, lime, strontia, and

baryta as regards their combinations with sulphuric acid, we find that the condensation of the elements increases from sulphate of magnesia to sulphate of baryta. It is least in sulphate of magnesia, that of these four sulphates which water dissolves easily; it is greatest in sulphate of baryta, which is quite insoluble.

In this respect all soluble sulphates are comparable to sulphate of magnesia; sulphate of lead, which is insoluble, resembles, on the contrary, sulphate of baryta.

The same relation is observed between chloride of silver, calomel, chloride of lead, and corrosive sublimate. The condensation of the elements is greatest in the first of these bodies, which is the most insoluble, and least in the last, which is most soluble.

Iodide of silver is more condensed than bromide, and this, again, than the chloride of the same metal—which agrees with their respective solubilities in liquid ammonia.

In the case of an acid soluble in water, the salts which it forms with bases, for the same state of saturation, are the more soluble the less the acid is removed from its primordial condition —that is, the weaker the condensation; and they are less soluble the stronger it is.

The phenomena of double decomposition are always determined by the production of the most condensed compound and by its precipitation.

Thus a greater force of union between the parts, the measure of which is their approximation (that is, their condensation), is a sign of insolubility, as Newton foresaw, a proof of increase in cohesion and a cause of double decomposition, as Berthollet taught.

But why is this condensation greater in the sulphates of baryta and lead, and less in the sulphates of magnesia and of copper? Why are the phosphates generally insoluble, while all the nitrates and all the acetates are soluble? We do not know; and if, to answer such questions, it is not, perhaps, necessary to arrive at an absolute knowledge of the nature of affinity, we must at any rate penetrate more deeply into its laws.

Lavoisier never stated fully his opinion on the subject of affinity. Newton wished, before investigating its nature, to make a thorough investigation of the laws which it obeys. But the restricted point of view chosen by these two great men gave place at the beginning of this century to a new point of view. They each of them compared chemical or molecular attraction to general attraction; Davy, Œrsted, Ampère, Berzelius, our colleague M. Becquerel, and their imitators endeavoured to connect it specially with electrical attractions, or even to identify it with these forces.

An electrochemical theory which could account for the effects

of affinity would have seemed impossible so long as statical electricity alone was known to physicists; but Volta's discovery and the investigation of the properties of dynamical electricity opened out a new path. It seemed natural to suppose, for example, that there was a close connexion, for instance, between the force of the spark which determines the combination of oxygen and hydrogen in the formation of water, and that of the battery which, effecting in silence and without intermission the decomposition of this liquid, transfers hydrogen to the negative and oxygen to the positive pole.

Reversing the decomposing mode of action of the pile, should we not obtain the most natural representation of the attractive force which unites the elements of water?

Davy was the first to endeavour to give by means of electricity an explanation of the permanent effects due to chemical attraction, and of the transitory phenomena which accompany the combination of bodies. He supposed that at the contact of an acid and a base their particles become charged with contrary electricities, and that at the moment of combination these electricities suddenly reunite. The compound formed, the light or the heat developed at the moment of combination are readily explained on this hypothesis. Davy supposes, then, that it is attraction which unites the particles of bodies, but that placing in contact sulphur and copper, for instance, they take opposite electrical conditions, that by heating them the electrical tensions are increased, that, lastly, the two electrical fluids acquire so high a tension that they attract one another and unite, producing heat and light, while the sulphur and the copper, being approximated by this contact, remain united by the attraction and thus form sulphuret of copper.

Ampère, modifying this hypothesis, regards the atoms as being endowed with an electricity of their own, and as being surrounded by an electrical atmosphere of the opposite kind. These electrical atmospheres, when they neutralize one another, produce heat and light; the electricities peculiar to the atoms produce the combination by their mutual action. Ampère has thus no need to bring into play general attraction; he refers to the operation of a single force both the transient and permanent phenomena of chemical action. But Ampère would willingly have sought in electricity the cause of universal attraction itself.

Berzelius, finally, regards the molecules as being not merely electrified but polarized.

These various conceptions have had only one single practical consequence. Davy, convinced that the force which united the elements of compound bodies was of electrical origin, concluded that, by opposing to the electricity of combination the electricity

of decomposition furnished by the battery, all bodies might be analyzed. Increasing, therefore, the voltaic power at his disposal, he succeeded in isolating the metals of the alkalies, those of the earths, boron, and silicon.

Since this great event electrochemical theories have taught nothing which could at all guide chemists either as to the nature of affinity, or the laws which regulate its influence in the formation or in the destruction of bodies.

It has been simply proved that every chemical action is accompanied by a movement of electricity, and that every conducting chemical compound may be disjoined when it is placed between the two poles of a battery. The metals are always liberated at the negative, and the oxygen at the positive pole, and other substances at one or the other pole, according to the nature of the compounds in which they are engaged.

When two bodies combine, electricity is disengaged ; and when two bodies separate, electricity is absorbed.

How much electricity is produced when two bodies combine ? How much is consumed in the separation of the same bodies ? These two questions have been attentively examined ; the discoveries of Faraday and of M. Edmond Becquerel on this important point, as well as Favre's researches in the same direction, have thrown a new light on them by defining in a precise manner electrical equivalents, but they have not furnished chemists with a doctrine of affinity.

Having perceived that the hope of representing affinity in its cause and in its effects as a purely electrical action was not realized, and led to no practical conception, I returned, in the last course which I had the honour of giving at the Faculty of Sciences, to the following point of view.

Accepting affinity as a fact, I proved :—(1) that combination seemed possible only in case the bodies placed in juxtaposition would disengage heat in acting on each other, but that in proportion as the combination became more complicated the heat disengaged became less; (2) that in order to separate combined substances, the heat must be restored which they had lost in combining.

Thus, taking as an example the formation and destruction of alum, I compared the following facts :—

Potassium and oxygen = potash	{	Brisk heat and bright light.
Aluminium and oxygen = alumina	{	Brisk heat and bright light.
Sulphur and oxygen = sulphurous or sulphuric acid	}	Heat and light.

Potash and sulphuric acid = sul-
phate of potash . . . . . } Heat.

Alumina and sulphuric acid = sul-
phate of alumina . . . . } Heat.

Sulphate of potash and sulphate of
alumina = alum . . . . } Heat.

Alum and water = crystallized
alum . . . . } Feeble heat.

Beyond this term combination becomes impossible, as we know; and crystallized alum appears the last product which can be realized with this order of compounds.

Conversely :—

Crystallized alum heated to 120° becomes anhydrous.

Anhydrous alum heated to redness is converted into sulphurous acid, oxygen, alumina, sulphate of potash.

Sulphurous acid, sulphate of potash, and alumina, when raised to extreme temperatures, are themselves converted into oxygen, sulphur, potassium, and aluminium.

The elements which combine lose heat. Therefore the elements of a chemical compound which separate must be raised to a temperature which is higher the greater the heat they have emitted in combining.

Heat being regarded as motion, combination would consist in a diminution of this motion; it would cease to be possible when the molecules of the compound had no more heat to lose.

Whatever be the manner in which this heat intervenes in the formation and destruction of chemical compounds, we must see in it the sum and the expression of all the forces put in play in the successive production of the various agglomerates of a compound, or for their disaggregation. And it was with a grand perception of the true nature of chemical phenomena that Lavoisier in his equations placed heat in the same rank as matter, and that he attached such great importance to the calorimetrical investigations which so long occupied him.

The extension which M. Regnault has given them, as regards specific heats, and that which they have received from M. Favre in all that concerns the disengagement of heat at the moment of combination, prepare chemistry for passing from the epoch in which it only considered matter to that in which it will take force into consideration.

The new researches to which the mechanical theory of heat has given rise have recalled the attention of chemists to the mechanical theory of heat stated by Jules Robert Meyer. This profound physicist considers chemical phenomena due to an attractive force which precipitates atoms against each other.

Their shock at the moment of contact would produce heat, light, electricity. The union of atoms once produced, in order to separate these, molecular forces must intervene capable of separating them and carrying them to the limit at which attraction, becoming null or even negative, would cease to act or be changed into repulsion.

Thus we are led to the simple views of Newton and of Lavoisier. Chemical combination takes place between ponderable bodies; the permanent effects are due to attraction; its transitory effects are due to the losses of motion which the atoms experience at the moment of their union.

However, general attraction being admitted as a necessary and sufficient representation of the force which determines chemical combinations, are we not led to efface the line of separation which has been assumed to exist between cohesion and affinity? Is it not convenient to see one and the same force varying its effects in the three states of aggregation—*cohesion, solution,* and *chemical combination*?

Not that we should confound them; for, their first cause being the same, it would be none the less indispensable to modify its application in these three circumstances, each of them having its own distinct and persistent character. Just as it would always be necessary to distinguish between general and molecular attraction, it would be none the less necessary to maintain the distinction between the three forms of molecular attraction. I have no doubt that, if once we knew the cause of affinity itself, we should recognize in its mode of acting on bodies well-marked modifications, as M. Chevreul long ago suggested.

Yet, if chemical action, the force of solution, and cohesion are mere modifications of general attraction, if they do not constitute so many special and distinct forces, ought we not to expect that the affinity of chemists more profoundly investigated would lose its special character, become more mechanical, approach little by little first its two congeners, and finally planetary attraction itself?

But cohesion and the force of solution, resembling in this respect general attraction, form continuous phenomena; the atomic theory, on the contrary, ranges affinity amongst discontinuous phenomena.

Berthollet, guided doubtless in this respect by Laplace, himself familiar with Newton's philosophy, maintained for a long time, as we know, that bodies can combine in all proportions. He would willingly have applied to chemical phenomena, and to the forces which determine them, Linnæus's axiom, *Natura non facit saltus*, which is true of organized beings; and if his opinion had been confirmed, affinity would have been attached more closely to cohesion and general attraction.

Proust, who maintained the contrary, succeeded in establishing his view.   Dalton's atomic theory, soon confirmed by Gay-Lussac's laws of gaseous combination, by Wollaston's experiments on salts in various degrees of saturation, by Berzelius's immense researches, and especially by the simple and constant ratios which he pointed out, in salts of the same acid and in the same state of saturation, between the oxygen of the base and that of the acid—all these striking events have powerfully contributed to lead chemists to continue to regard affinity as having a character of its own and as having almost nothing to borrow from general attraction ; for, in fact, what links are to be established between general attraction (acting directly as the masses and inversely as the square of the distance, obeying without discontinuity all changes in mass, all changes in distance) and chemical affinity ?

Viewed with regard to masses, affinity does not admit that combination can neither be effected below a certain minimum nor above a certain maximum.   Between these two extreme limits the atomic theory, confirmed in this respect by the universal experience of chemists, just as little allows that combinations may be indefinitely multiplied ; far from that, it limits the number, and only admits those which are represented by atoms united in simple ratios, represented by whole numbers, as $1:1$, $1:2$, $2:3$, $2:5$, $2:7$.

If it be true that the experiments of MM. Marignac and Debray have rendered certain the existence of compounds formed in accordance with more complex ratios, even in mineral chemistry, still nothing indicates that their formation takes place according to a law of continuity, and that in this respect they disaccord with the fundamental principle of the atomic theory.

Thus the reciprocal action exerted by the atoms of bodies, attractive at inappreciable distances, less so as they separate, becoming zero, or even repulsive when the sign is changed—this, according to Newton, is the most faithful image of affinity.

But to make the effects agree with the incontestable results on which the atomic theory is founded, we must add, with Newton, that the figure of the atoms should be taken into consideration. It is not difficult to understand, in fact, that the action exerted by the peculiarities in the shape of the atoms may limit the production of their compounds, and restrict them to uniting in simple ratios expressed by whole numbers.

Ampère in his youth had proposed to the chemists of his time a doctrine of chemical combination which both appealed to the principles of the Newtonian attraction and the laws of crystallography : it excited but little interest ; it represented neither the absolute ideas of affinity as then understood, nor the ideas of chemical dualism as the interpreters of Lavoisier understood it,

or at any rate those who, giving to his nomenclature all the force of a doctrine, had seen, in the creation of a language made to aid the memory by logic, a real representation of the intimate constitution of compound bodies.

Such, in fact, is the power of the forms of language, that it is necessary to make an effort over one's self to understand that in an oxide or in a sulphuret, for instance, it may be that the metal is not the body overcome, conquered, subordinated, and that oxygen and sulphur are not the dominant bodies. In the same way in salts. The French nomenclature, irreproachable so far, that it limits itself to making known the nature of bodies united to form a compound, has never attempted to define the arrangement they affect in the combination once it is formed. To give it this meaning is to falsify it and destroy its real use.

The French nomenclature was intended to interpret a natural classification. It first discriminated elements and compound bodies. In the latter it has formed genera, and characterized species. The genera have been defined by the element common to all the species—oxygen for oxides, sulphur for sulphurets, carbonic and nitric acids for carbonates or nitrates; the species by the substance which forms the complement of the compound:—oxide of iron, of zinc; sulphuret of lead, of silver; carbonate of lime, nitrate of potash.

The French chemists have proceeded as naturalists; and as they created a new language, they have been able to make the names of the genera singularly significant by varying the terminations.

But there is nothing, either in this new language or in the interesting exposition of it in which Lavoisier has laid down its origin and its principles, to indicate that on his part and that of his co-workers there was any other object than that which has been mentioned—to range together compounds which have an element in common, to indicate what substances enter into each combination, and in what proportion. The idea of a molecular arrangement, of an intimate constitution, of the compound was never entertained.

At the present time we should be disposed to admit that the theory of chemical combination proposed by Ampère agrees best with the general laws of mechanics, for it depends upon universal attraction—and with the special laws of chemistry, for it brings in as the determining and characteristic element of the constitution of bodies the shape of their molecules, which would contain at least four atoms each where they are tetrahedral, and as many as fifty atoms for other solids.

It would be unjust to omit the mention of M. Gaudin's name by the side of that of Ampère. The efforts of this ingenious

philosopher, whatever idea be entertained on the subject of their import and of their future, have had at any rate this result, that they have led chemists to take into account certain laws of symmetry in arranging the formulæ of compound bodies. The corrections based on them, proposed by M. Gaudin forty years ago, have been confirmed by experiment; they have paved the way for the rearrangement of the formulæ of all compounds of carbon or of silicon—that is to say, of all compounds of organic origin, and of almost all those the investigation of which constitutes mineralogy properly so called.

The absolute defence of dualism retained no partisans after the last struggles which Berzelius maintained with singular brilliancy towards the end of his life in the interest of this doctrine.

It is to be observed that the electrochemical theory, regarding the elements of bodies as obeying the two electrical forces, and the materials of every compound as uniting in twos to form by successive agglomerations, and always two by two, gradually more complicated combinations, proceeded in harmony with the French nomenclature. It is not, therefore, surprising that the use of the molecular system proposed by Ampère, modified by M. Gaudin, and generally adopted with divers variations by the chemists who, being occupied with organic chemistry, are obliged to take account of the phenomena of substitution, has both rendered less ardent the pursuit of a precise electrochemical theory, and less confident the too absolute interpretation of the French nomenclature.

We are thus led to conclude that, on the one hand, the search for an electrical theory of affinity now occupies but few—although beautiful and useful applications of electro-chemistry have been accomplished of late years, and are pursued with great success; on the other hand, that it is no longer possible to represent by means of the dualistic nomenclature the multitudinous chemical compounds which the molecular system registers daily.

We are thus more and more led towards the experimental investigation of chemical types as a basis of the classification of compounds, apart from any hypothesis on the internal arrangement of their elements,—which constitutes the true foundation of the French nomenclature properly understood.

At the same time we are led towards the idea which attributes to the molecules of compound bodies a more complex constitution than would be derived from the binary nomenclature, and which makes of them planetary or crystallographical systems presenting several centres of force—mobile in the first case, fixed in the second.

We finally revert to the thought which would directly connect affinity with universal attraction.

In recent times the views of Newton have met with an unexpected and remarkable support in the beautiful and important researches which our eminent colleague M. Henri St.-Claire Deville has devoted to the phenomenon of dissociation—one of the greatest acquisitions, not only of chemistry, but of natural philosophy.

Nothing, in fact, is more in accordance with the laws of mechanics than to say that a volatile liquid placed in connexion with a free space fills it with its vapour, and that the tension of this increases or decreases regularly in a continuous manner according as the temperature rises or falls.

But to say that carbonic acid separates from lime in the same manner, to establish that there is a tension of decomposition analogous to the tension of vapours, and that the evaporation of a liquid and the decomposition of a carbonate take place in virtue of the same laws, and present the same phenomenon of continuity, is to connect chemical combination with cohesion, is to prove that under certain conditions the laws which regulate the aggregation or the separation of molecules of the same kind are also applicable in the case of molecules of different kinds.

Without affirming that in all cases the dissociation of compounds presents the character of a continuous phenomenon, it is enough that the case is frequent (as is proved by the researches of our eminent compeer and those of his pupils), to justify our assimilating the purely physical molecular separations and the chemical molecular separations, and to give us the right henceforth to unite cohesion and affinity the one to the other, and both to universal attraction.

It follows from this brief summary :—

(1) That Newton gave of chemical affinity a notion to which nothing has been added, when he connected it with general attraction and showed how, at a certain distance from the centres of molecular action, it may become zero, or even repulsive.

(2) That Ampère has given the complement to this view by showing that the shape of the components limits the number of combinations which two elements may produce, and that it determines the ratios according to which they may unite, and even enables us to predict the ready replacement of one element in a complex molecule by another without its stability being compromised.

(3) That Meyer has shown how the impact of molecules, striking against each other with extreme velocity to produce combination, may give rise to the phenomena of heat, of light, and of electricity which accompany chemical action.

(4) That M. Henri St.-Claire Deville, in discovering the capital phenomenon of dissociation, has opened a new way to science

by closely connecting chemical combinations with the purely phy-
sical phenomenon of the formation of vapour.

(5) Finally, that the doctrines with the aid of which it has been
attempted to explain chemical phenomena by a distinct unknown
cause, or by electricity, have yielded no fruit ; while those which
tend to bring it under the laws of universal attraction consoli-
date themselves, approach more and more to the facts, and indi-
cate better and better the path of progress.

It would then be natural, just, and useful that Newton's
name, and the definitions he gives, both of molecular attraction
and of chemical atoms, should be retained in works intended for
instruction in chemistry.   In my opinion (but I give it with all
the reserve which such subjects demand), heat is the real measure
of chemical energy, while light and electricity may for the pre-
sent be regarded by the chemist either as agents of which he
makes use, or as phenomena whose appearance he ascertains.

Matter and *heat* would still be, as in Lavoisier's time, the de-
finition of the two objects to which the thoughts of chemists
should especially be directed.

The time will doubtless come when, the laws which molecular
attraction obeys being themselves known, we shall be able to
predict or explain the formation of compounds, their destruc-
tion, the preferences and the choice of elements in the formation
of combinations, the special affections which acids or bases exhibit
in the production of salts ; but before attacking this last and
difficult problem, we must know otherwise than by suppositions
the bond which connects the shape of the crystals of one chemical
species with the arrangement of the atoms the grouping of which
constitutes the molecules which are the materials of these
crystals.

I hope the Academy will pardon my having so long occupied
it with these historical considerations, and that it will understand
the interests attached to them.

My object would be attained if, on the one hand, I had con-
tributed still more vividly to direct the attention of chemists to the
relations which connect the motions of heat with the transfor-
mations of matter, and if, on the other, I had shown that the
principle of the French nomenclature is not unsuited to the
classing and naming the compounds of organic or molecular
chemistry.

Lavoisier, in proposing the new nomenclature, stated that the
Commission, of which he was the eloquent organ, " had been un-
willing to devote itself to great discussions on the constituent
principles of bodies and on their elementary molecules—that it
had severed itself from the systematizing chemists, who are always
ready to accompany facts by an apparatus of reasoning in which

the fact itself disappears, and in whose hands science becomes an edifice raised by their imagination."

He declared, finally, that the Commission " had sought to ap- ply to chemistry the logic which belongs to all sciences—the name of *class* or *genus* recalling, in the natural order of ideas, properties common to a great number of individuals, and that of *species* properties peculiar to certain individuals."

I do not know whether I am deceiving myself; but it seems to me, moreover, that this duel of antagonist molecules which is met with in all the phenomena of chemistry, and which the pre- sent nomenclature expresses so well, remains incontestable, and that we should not give up depicting it until we are forced to do so. But the act of combination once accomplished, the duel ter- minated, the French nomenclature does not pretend to say that the two bodies which have acted on each other have retained their distinctive character in the molecule formed and are not con- founded in a complex system. It is in this respect that Berze- lius, going beyond Lavoisier's idea, exaggerated the meaning.

It is not without a legitimate satisfaction that we have the right to say in this circle, that, notwithstanding the progress which has metamorphosed the field of chemistry, the Academy has nothing to regret, either as regards doctrine or language, of what our illustrious predecessors had founded with so much prudence, wisdom, and even genius.

XIII. *On the Heating of a Disk by rapid rotation* in vacuo.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

WITH reference to the paper of Herr O. E. Meyer, which you translated in the last Number of the Philosophical Magazine, we desire to make the following remarks.

There was no *assumption* (Annahme) whatever in our statement of the small but unavoidable deviation of the axis from perpendi- cularity to the disk. The amount of this bias was, in each case, directly *measured* by turning the disk so slowly that no flexure could possibly be produced by the rotation ; and the amount thus determined was not visibly exceeded even at the highest speeds. The length of the axis is nearly 4·5 inches (or more than two- thirds of the radius of the disk) ; and it lies in two bearings which fit it as tightly as is consistent with free rotation. The utmost amount of deflection of the edge of the disk due to slackness of these bearings cannot possibly be nearly as great as 0·001 inch.

Herr O. E. Meyer supposes, contrary to the usual principles of ordinary dynamics, that a change of position of the instanta-

neous axis of the disk necessarily implies loss of *vis viva*, and he calculates the supposed loss in a very peculiar manner. What he means by saying that *vis viva* lost by impact of the axle on its bearings is employed in heating the disk (alle übrige lebendige Kraft geht für die Rotation verloren und wird zur Erwärmung der Scheibe verwandt) we cannot pretend to understand. Such a statement, if meant to be understood literally, would appear to be contrary to the fundamental principles of thermodynamics. If, however, though he certainly does not say so, Herr Meyer means that impact of the axle on the bearings *may produce vibrations of the disk* which in time will by viscosity be frittered down into heat, he merely repeats one of many objections, long ago perceived by ourselves, and also pointed out to us by others, an objection which we have already at least partially met by experiment and calculation. [It may be well to say here that Professor Helmholtz, nearly two years ago, very kindly *detailed* to us his objections, making several valuable suggestions by which we have since endeavoured to profit, and which will be duly acknowledged when we are in a position completely to answer these objections. This will, we hope, soon be the case, as extensive additional apparatus is now in course of construction.]

There are various other parts of Herr Meyer's paper to which we might easily take exception, especially the calculation he makes (even supposing his assumptions to be correct) as to the absolute amount of radiation to be expected; but it would be foreign to our present object to enter into such details. The determination of radiation in absolute measure is an inquiry of great importance, and we are glad to hear that it has been taken up by Professor Neumann; we believe that Sir W. Thomson also has been working at it; so that the question is now in good hands, and will no doubt soon be definitely answered.

<div align="center">We are, Gentlemen,</div>

<div align="right">Yours &c.,
B. Stewart,
P. G. Tait.</div>

January 1869.

<div align="center">

XIV. *Fundamental Principles of Molecular Physics.* *By* Professor W. A. Norton*.

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IN a recent work by Joseph Bayma, S. J., Professor of Philosophy, Stonyhurst College, England, in which a new theory of Molecular Mechanics is ably set forth, I find a brief critique of my theory of Molecular Physics, published originally in Silliman's Journal, and republished in the London and Edinburgh

<div align="center">* From Silliman's American Journal for September 1868.</div>

Philosophical Magazine. To this I propose to reply, and at the same time to remark incidentally upon some of the fundamental principles of the theory advanced by Professor Bayma. This can be most briefly and effectively done by taking up the different objections urged by him in due order, and commenting upon them in succession, having a care not to dissociate remarks that should properly be presented together. The quotations made will be indicated to the eye by being printed in smaller type.

A great number of scientific men, to give an explanation of calorific, electric, and luminous phenomena, assume that æther pervades all ponderable bodies; whence many of them have come to the conclusion that every molecule of a body is surrounded by an æthereal atmosphere, the action of which is considered to be the source of those phenomena. Professor W. A. Norton, in a series of interesting articles published in the American Journal, gives a theory of molecular physics, of which the fundamental principle is that each molecule is formed by an atom of ponderable matter surrounded by two æthereal atmospheres of a different kind. I give his words.

" The established truths and generally received ideas which form the basis of the theory are as follows :—

" 1st. All the phenomena of material nature result from the action of force upon matter.

" 2nd. All the forces in operation in nature are traceable to two primary forces, viz. attraction and repulsion.

"3rd. All bodies of matter consist of separate indivisible parts called atoms, each of which is conceived to be spherical in form.

" 4th. Matter exists in three forms essentially different from each other. These are (1) ordinary or gross matter, of which all bodies of matter directly detected by our senses either wholly or chiefly consist. (2) A subtile fluid, or æther, associated with ordinary matter, by the intervention of which all electrical phenomena originate or are produced. This *electric æther*, as it may be termed, is attracted by ordinary matter, while its individual atoms repel each other. (3) A still more subtile form of æther, which pervades all space and the interstices between the atoms of bodies. This is the medium by which light is propagated, and is called the *luminiferous æther*, or the *universal æther*. The atoms, or ' atomettes ' of this æther mutually repel each other; and it is attracted by ordinary matter, and is consequently more dense in the interior of bodies than in free space.

" 5th. Heat, in all its recognized actions on matter, manifests itself as a force of repulsion.

" The corner stone of a physical theory of molecular phenomena must consist in the conception that is formed of the essential constitution of a single molecule—understanding by a molecule an atom of ordinary matter, endued with the properties and invested with the arrangements which enable it to exert forces of attraction and repulsion upon other molecules. In seeking for this, the most philosophical course that can be pursued is to follow out to their legitimate conclusions the general principles already laid down. . . . . . The

conception here formed of a molecule involves the idea of the opera-
tion of the two forces of attraction and repulsion : a force of attrac-
tion is exerted by the atom upon each of the two atmospheres sur-
rounding it, and a force of mutual repulsion between the atoms of
each atmosphere.   These we regard as the *primary forces* of nature,
from which all known forces are derived."

These are the capital points of Professor Norton's ingenious
theory.   But we think that such a theory contains a great deal of
arbitrary assumption.   And indeed on what evidence are we to
grant that matter exists in three forms essentially different from each
other ? Then how can we know the existence of atoms of gross
matter having a spherical form, and therefore extended though indi-
visible ? Why should we admit two æthereal fluids, which are both
repulsive and only differ in subtilty ? All this the learned Professor
assumes without proof, apparently because it consists of " established
truths and generally received ideas."   But we say that no one has
up to this day established the proof of such propositions.   As for
" received ideas," every one knows how often questionable notions
have been and are received without serious examination, especially
when expressed by professors in a very dogmatic style.   Are not a
thousand hypotheses received ? and do they cease to be hypotheses,
although he who makes use of them for building a theory adorns
them with the high name of principles ?

This is all very plausible, but the objections urged are desti-
tute of any real force. We will first consider the general intima-
tion that the theory rests upon " a great deal of arbitrary
assumption."   No theory of molecular physics can, in the
nature of things, have any other foundation than general prin-
ciples to be regarded as hypotheses that have been rendered
more or less probable, either by inductions from observation or
by *à priori* reasonings.   Molecular physics cannot be erected,
like mathematics, upon a foundation known from the first to be
eternally sure, that of self-evident truths.   Mechanical axioms
may exist as mere figments of the mind, and have often risen
like bubbles in the minds of speculative philosophers, shone with
an evanescent splendour, and suddenly burst at the touch of a
hard fact.   Our author is another instance of a learned philo-
sopher who has faith in such unsubstantialities, and thinks to
substitute them as a proper basis for a theory of molecular me-
chanics, in place of the general conceptions to which the pro-
gress of science leads, and by which alone its highest inductions
find any explanation—regards the latter as arbitrary assump-
tions, and his own mental convictions of what matter must be
and how it must act as the only reliable foundation upon which
to build.   It is true that he takes exception to Principles 3rd and
4th from the inductive point of view.   Upon this ground (the
only legitimate one to be occupied) I am quite ready to meet

him ; but I wish to enter here, at the outset, a demurrer against the virtual claim of the superiority of his own *à priori* method of establishing his fundamental principles. Such a claim is implied in the intimation that " no one has up to this day established the truth of such propositions," as will be best appreciated by those who have read Professor Bayma's book. Having proved, as he conceives, his propositions, and clinched each one of them with a Q. E. D., he insists that obvious intimations of nature are to be discarded because the stamp of infallibility cannot be put upon them at once, before the test of availability in the explanation of phenomena has been applied. It was evident from the tenor of my exposition of the subject that the " established truths " re- ferred to were merely regarded as having been virtually estab- lished, or rendered highly probable, by the inductions of science. The claim implied in Professor Bayma's criticism, that they re- quire a higher confirmation, in fact a demonstration of their truth, is not to be admitted.

He asks :

On what evidence are we to grant that matter exists in three forms essentially different from each other ?

A sufficient answer to the critic himself is, that, pursuing a systematic course of deduction from his leading principles and his assumptions of the essential nature of matter, he actually proves to his own satisfaction that matter does in fact exist in essentially three different forms. He reaches the conclusion that every primitive molecule consists of an attractive nucleus surrounded by a repulsive envelope. My own position is that every primitive molecule consists of an attractive atom of gross matter surrounded by a repulsive atmosphere of electric æther. The atom of gross matter answers to his attractive nucleus, the electric æther to his repulsive envelope. The difference of doc- trine, from the present point of view, is in name only. In an- other connexion he elaborately undertakes to prove that æther (*i. e.* the æther of space) is a " special substance." Thus he makes out that there are three essentially different forms of matter.

But to reply to others who may be disposed to adopt the ob- jection urged. No one will deny the existence of gross or pon- derable matter, or of something which has all the mechanical attributes of matter. That an æther exists in space and within transparent media we may certainly regard as abundantly estab- lished by optical phenomena. As to the electric æther, the evi- dence of its existence is that the great body of electric and mag- netic phenomena, it is generally conceded, admit of satisfactory explanation on the hypothesis of an electric fluid or æther inti-

mately associated with matter, and that no successful attempt
has yet been made to account for the simplest of these phenomena
on any other hypothesis. Some physicists, it is true, are stri-
ving to do away with the supposed electric fluid—prompted by
the conjecture that nature must operate by some simpler method,
and work out all her wonderful diversity of phenomena by one,
or at most two forms of matter. Shall we wait until these
physicists have realized their aspirations, at their discouraging
rate of progress ? or, guided by the indications of nature, strive
to link all natural phenomena together by a few recognized
principles ? A theory that shall accomplish this is the great de-
sideratum. Even should such a theory not rest upon the highest
and fewest possible mechanical principles, still the generalizations
embodied in it must have their counterparts in certain physical
truths, to the knowledge of which it will be likely to lead. It
is by following the ascending grade of generalizations that spe-
culative science has hitherto progressed. Preconceived notions
of what matter must be in its essential nature, or by what form
of matter or varieties of method nature must operate, have thus
far contributed little to its advancement ; and in fact, when we
consider that we positively know and can know nothing *à priori*
with regard to the essential nature and condition of matter, and
its means and mode of operation, such notions are entitled to
little credit.

Our author implies in the remarks above quoted that the ex-
istence of an electric æther is not only not an " established truth,"
but is to be ranked among those questionable notions that have
been received without serious examination. This implication is
obviously unjust. Besides, the serious examination that he has
given the subject only leads him to confirm the substantial truth
of what he would here seem to discredit ; for, as we have already
seen, his " repulsive envelope " is essentially my " electric at-
mosphere."

Why should we admit two æthereal fluids which are both repul-
sive and only differ in subtilty ?

Professor Bayma and myself agree in admitting the existence
of two *kinds* of matter, attractive and repulsive ; and, as we have
seen, three *forms* of matter. Is it inherently any less probable
that two of these should be repulsive and one attractive, than, as
he assumes, that two should be attractive and one repulsive ?
viz. gross matter and the æther of space attractive, and the
elements of the " repulsive envelope " repulsive. In the
supposition that the two æthereal fluids differ in subtilty,
nothing more is essentially implied than that a considerable
number of atoms of the one occupy the interstices between the

atoms of the other. Professor Bayma assumes equally great differences to subsist between his two attractive forms of matter. He remarks, " the distinction of such a medium " (a medium for the transmission of light) " from any ponderable substance is not an hypothesis, but a necessary inference drawn from observed facts ;" and again, " I do not see how such a fact " (that light can pass undisturbed through air notwithstanding the immense number of air-particles it encounters) " can be accounted for if æther is not immensely denser than atmospheric air." The reason for the conclusion is groundless ; but it is the conclusion itself that we have here to notice. He adds, " with this great density æther possesses also a very great subtilty."

I might also reply to Professor Bayma by asking him why we should admit, in order to explain electric and optical phenomena, two substances so distinct as the repulsive envelope of molecules and the attractive luminiferous æther. The evidence of their similarity is much greater than of their dissimilarity.

In speaking of the two æthers as subtile, it was meant that a large number of their atoms occupied the interstices between the atoms of gross matter. It was also, of course, recognized that the velocity of propagation of a wave is much greater through either of the æthereal fluids than through a mass of ordinary matter. The only apparent force in the question under consideration is derived from the fact that a vague conjecture is apt to be raised by it, that a single æther may be equal to all the duty now assigned to both.

To proceed with our quotations :

What we have said on the constitution of molecules demonstrates indeed the necessity of granting to each molecule of ponderable matter a repulsive atmosphere, which we have called the *molecular envelope*. But this envelope is not of æther, since æther is not repulsive.

That is, is not of the same substance as his luminiferous æther, which he regards as attractive. But the " atmosphere " which corresponds in its direct operation with Professor Bayma's " molecular envelope " is composed of electric matter, and this is repulsive. It is true that I conceive the interstitial spaces of this electric matter and the space between it and the central atom to be pervaded by the æther of space ; but the mechanical part chiefly played by this condensed universal æther consists in its being the medium in which pulses are originated that constitute the force of *heat-repulsion*.

Had Professor Norton known the impossibility of continuous matter, he would have found out that what he calls an atom of gross

matter comprises already not only the central element of a molecule, but its nuclei and its envelope ; and consequently is already endued with the properties and invested with the arrangements which enable it to exert forces of attraction and repulsion upon other molecules, without requiring any new and special atmosphere of electric or luminiferous æther.

That is, in other words, as already shown, Professor Bayma's nucleus and envelope are in all outward relations precisely correspondent to my central atom and electric atmosphere. The only essential point of difference between us lies in the fact that I conceive that the interstitial luminiferous æther is condensed around the central atom, and is concerned in the production of some of the phenomena. It is not easy to see how Professor Bayma escapes the conclusion that his interstitial æther, which is attracted by the central nucleus, is condensed around it ; still it is plain that he imagines that all natural phenomena are produced by the mutual actions of molecules composed of a central nucleus and a single repulsive envelope without the intervention of any æther, except the luminiferous in the case of the phenomena of light and radiant heat. This will appear from the following quotation :—

As for the examples by which he illustrates the theory, they consist of a series of phenomena of different kinds, the explanation of which does not show that the theory is not at fault. For it must be remarked that those explanations do not imply the existence of extended atoms or of two distinct æthereal substances; and therefore the theory assumes more than is necessary for, or guaranteed by, the explanation of phenomena.

When he has shown this to be true of even the ordinary calorific and electric phenomena, we will admit that his objection to a second æthereal atmosphere interpenetrating the first may have some force. He has given no hint of the general manner in which he supposes electric phenomena to be evolved. Heat he conceives to originate in the vibrations of the molecules of bodies ; but it can be proved, almost to a demonstration, that heat cannot originate in this manner.

Our author proceeds as follows :—

The atoms of gross matter being "indivisible" cannot be extended, and cannot be conceived to be "spherical in form ;" for if they were extended and indivisible they would be so many pieces of continuous matter, which we have already proved to be impossible.

To this I have the following replies to offer :—

1. Professor Bayma assumes that every point of matter acts instantaneously upon every other point at all distances, however great or small, with a force having the same character at all dis-

tances, and inversely proportional to the square of the distance. This may seem probable, but is not self-evident ; and in fact no reason can be assigned why one material point having no extent should act upon another with a force decreasing with the distance, according to any law whatever. The law of inverse squares is a consequence of wave propagation, or of radiations along definite lines, received on a molecule of definite size, and cannot be predicated of a force that acts instantaneously between two mathematical points. To suppose such a law is an arbitrary assumption.

2. If matter consists of material points, as supposed by Professor Bayma, it is no more difficult to conceive of an atom of continuous matter than of the space coextensive with it.

3. It is not more difficult to conceive of an indivisible atom acting as a whole upon another atom with a certain energy, than of a mere point acting upon another point, and causing it to change its place, at the same time transferring to a new point all the properties it possesses.

4. If the occult nature of the force of action of one material point on another be such that the intensity becomes indefinitely small at indefinitely small distances, instead of infinitely great as imagined by Professor Bayma, then a collection of an infinite number of material points may form one invariable atom, since the size of the atom may in every instance be so inappreciable in comparison with the distance between the nearest atoms that there may never be any inequality of extraneous action on different points of the same atom, imparting different velocities to them, and so tending to break up the continuity of the matter. Besides, we have already seen that no inequality of elementary action, by reason of a difference of distance, is legitimately deducible from Professor Bayma's premises.

5. In speaking of atoms of gross matter as " indivisible," no other ground was intended to be taken than that each atom was indestructible from any possible action of another atom, and essentially invariable in form. This does not preclude the idea that the atom may be an aggregation of a finite number of material points ; for it may be that the mutual action of two attractive points passes into a repulsion at excessively minute distances, and so that an atom of ordinary matter may be a system of material points in either a statical or dynamical equilibrium. Indivisibility, taken in the only sense in which the term can properly be used, does not, then, necessarily imply continuity, as maintained by Professor Bayma.

6. The assumption that each atom is " spherical in form," was adopted merely as the simplest embodiment of the fundamental principles that the action of the atom was equal in all

directions, and that the attractive action upon an atom of æther was neutralized at minute distances by the resistance developed at the point of contact. The existence of such a resistance necessarily implies that the elementary parts of the attractive atom, whether finite or infinite in number, act repulsively at very minute distances. But another conception may be formed of the mode of operation of an atom of gross matter, which involves no other supposition than that it acts equally outward in all directions from a centre, and takes no account of its geometrical extent. This is, that *the effective attraction of the atom for the æther of space is due to the existence of a repulsion less than would be exerted by the one or more atoms of æther that would naturally occupy its place.* The result would be the condensation of an atmosphere of æther around the atom, without the exertion of any direct attractive force, or of any additional force of resistance. We may conceive the molecular atmosphere of electric æther to originate in a similar way; but as the opportunity of examining and testing this idea sufficiently has not yet been obtained, I shall continue to regard the electric æther as directly attracted by the atom of gross matter, and that the antagonistic force of resistance is furnished by the repulsion of the luminiferous æther condensed around the atom.

If, in accordance with these views, we seek for a possible origin of *gravitation,* we can find it in a primary attraction subsisting between atoms of gross matter. This must be excessively feeble in comparison with "molecular forces," and modify the effect of those forces only by creating a slight additional pressure of contiguous molecular atmospheres. Should we assume the primary actions between atoms of all kinds to be wholly repulsive, and the effective attraction of the gross atom for both its æthereal atmospheres to be a mere consequence of inequalities of repulsion, it is conceivable that the attraction of gravitation might result from æthereal waves, as maintained by Professor Challis, these waves having their origin in a *dynamical* equilibrium of the atmosphere of universal æther condensed around each atom.

Another critical remark is the following:—

Again, æthereal substance, according to the author, is repulsive; now this is inconsistent with astronomical facts, as we have sufficiently shown.

The principal astronomical fact here referred to is that the planets do not encounter any sensible resistance in their motion through space. The evidence of an æthereal resistance afforded by Encke's comet, Professor Bayma strives to explain away without success. The fact that no sensible resistance is experienced

by the planets does not necessarily imply, as he supposes, that the æther is not repulsive. For, in the first place, if the molecules of the planetary mass have the constitution I have attributed to them, the impinging æther must take effect upon either the æthereal or the electric atmospheres of the molecules, and so may be mostly expended in the generation of heat and electric currents. I have in fact undertaken to show, in my paper on Molecular Physics, that the earth may derive its magnetic condition and a certain portion of its heat from the impact of the æther of space. Again, if the action of gravity be not instantaneous, it will take effect in a direction slightly inclined to the radius vector, and, in the existing state of the planetary system, the tangential component resulting from this inclination may be in equilibrium with the feeble overplus of resistance from the æther. Besides, the supposed difficulty is not removed by substituting an attractive for a repulsive æther. It is true that when a molecule of the earth's mass encounters an atom of the æther on the line of its advance, it will, upon Professor Bayma's idea, pass through it and leave it behind; but he has failed to note the fact that during the approach of the two their relative velocity will be equal to the sum of the velocity of the earth and that due to their mutual attraction, and during their separation will be equal to the difference of the same velocities, and hence that the atom of æther will continue to attract the molecule during a longer interval of time while the two are separating than while they are approaching. The molecule will therefore on the whole be retarded by the action of the atom. If the attractive æther be " immensely denser than atmospheric air," the resistance should certainly not be less than that of a subtile repulsive æther. If Professor Bayma should still hold to the same line of argument, I do not see but he must abolish the æther of space altogether.

He continues:

Moreover the writer, after having assumed that the electric and luminiferous æthers are both made up of atoms that repel each other, assumes also that electric æther attracts luminiferous æther; for he admits that a molecule is formed of an atom of gross matter with two atmospheres, of which the first, consisting of condensed luminiferous æther, is attracted by the æther which consists of electric æther. Now, if the atoms of electric æther are repulsive, how can they attract? So, then, we must conclude that Professor Norton's theory, as presented by him, in spite of the talent and learning of its author, cannot be adopted in science.

Professor Bayma has here entirely misunderstood me, and represented what I threw out as a possible and perhaps probable

conception, to be a fundamental principle of my theory. The real fundamental principle was that the atoms of electric æther repelled each other ; and it was merely conjectured that this repulsion might be due to atmospheres of luminiferous æther condensed around the electric atoms, instead of being a repulsive action. It is a little singular, in view of this distinct statement of the manner in which the repulsion might result from a possible attraction, that our author should ask the question, " Now, if the atoms of electric æther are repulsive, how can they attract ? " and thereupon intimate the existence of a discrepancy fatal to the theory. It is, in fact, altogether immaterial whether the mutual repulsion of electric atoms is indirect as conjectured, or direct.

It has now been made sufficiently apparent that the objections urged against my theory of molecular physics have no real force, and that its fundamental principles have not been disturbed. Whether it will ultimately be " adopted in science " or not must depend upon its availability in rendering a satisfactory account of phenomena, and its ability to withstand the test of a detailed comparison with the entire range of physical facts. If life and health are granted me, I shall endeavour in good time to show, to the satisfaction of every candid mind, that the natural phenomena and experimental results, with their laws and features of diversity, that make up the different departments of physics are legitimately deducible from the fundamental principles of the theory, and that it presents claims to acceptance superior to those which can be urged in favour of any other theory.

The attempt to deduce the existing constitution of things and prominent phenomena by Professor Bayma from his fundamental ideas, so far as made, has certainly failed at several important points. To specify one or two of these. He obtains a curve of molecular action that represents a repulsion at the smallest distances succeeded by an attraction at greater distances. This can only be made to represent the three states of bodies by conceiving the molecules of a gas to be in such a condition that, if it were entirely freed from pressure, it would expand into a liquid. We know that many gases can be compressed into a liquid, but it is altogether gratuitous to suppose that they could be brought into a similar condition by a diminution of pressure. Experiment has given no indication of such a result or tendency.

Heat and light he conceives to originate in vibrations of gross molecules ; but against this notion, as I shall take another occasion to show, insuperable objections may be urged. If this be given up, his explanation of the changes of the state of bodies must also be abandoned.

The doctrine that " transparent bodies transmit rays of light

by the motions of their own molecules " will hardly be accepted, we think, by physicists. It would be a waste of time to argue against it.

Again, the notion that a certain substance radiates light of a certain colour because its molecules are made to vibrate in unison with the ray of that colour, will not stand ; for the results of spectral analysis show that the parts of a body which are capable by vibration of giving out any colour are precisely those which absorb and stifle that colour. This fact, we may add, also proves conclusively that the rays cannot be transmitted by the motion of the molecules. Though so radically at variance with Professor Bayma's theoretical views, it is in entire accordance with my own ; for, according to these, light originates in certain vibratory movements of the atoms of the electric atmospheres of molecules, and when these vibrate naturally in unison with the ray of any colour that falls upon them, they take up its *vis viva,* and so the ray is transformed into a molecular electric current.

As to the " leading principles " laid down by the author, they may in the main be conceded ; but these by no means cover the whole ground upon which his theory is raised. We find, for example, that he assumes that all elements or material points of the same form of matter act, under similar circumstances, with the same intensity. Now if this principle be admitted, what theoretical basis have we for the existence of distinct primitive molecules for every different substance, the number of elements associated together being exactly the same for each primitive molecule of each substance, and different for primitive molecules of different substances ? The natural tendency would be to a fortuitous association of elements in an endless variety of numbers into groups. No controlling principle by which uniformity would be evolved from chaotic confusion is furnished by the theory. The Hand of the Creator must be supposed to have miraculously interfered, and guided each element to its precise place in the formation of every molecule of matter. The objection here urged derives still greater force from the consideration that both the nucleus and envelope of each specific molecule are assumed to have a regular geometrical form, different for each substance. To assume the existence of such molecules is to make an incalculable number of arbitrary assumptions. No such exception can be taken to the views I have advocated ; for primarily each specific atom of gross attractive matter must appropriate to itself, from the universally diffused repulsive æthers, its electric and its æthereal atmosphere, each of a certain definite extent. Upon the relations of these specific atmospheres to the central atom and to one another, all the different properties of each specific molecule must depend.

We have already seen that the principle that one material point acts upon another instantaneously, without the intervention of any medium, is opposed to the fundamental idea that the force exerted is inversely proportional to the square of the distance. This law, to say the least, is an arbitrary assumption in the premises. The author also conceives that the mutual action of two material points is in no degree and under no circumstances intercepted by another intervening point. But we know that, in the case of the molecular forces, the amount of *vis viva* expended in imparting motion to one particle is abstracted from the force in action ; and, according to Professor Bayma, the molecular forces are of the same nature as the forces subsisting between the material elements. The force of gravity, it is true, is not sensibly intercepted ; but this does not prove that a tendency to interception does not exist ; for, upon the supposition of a wave-transmission of the force, the effective attraction of any molecule may be the mere differential of the actual force transmitted, and, besides, in the circular revolution of a planet the distance from the sun remains unchanged.

My own doctrine is that the molecular forces, including the heat-repulsion, are dynamical forces transmitted by wave-propagation and developed by the primary forces of attraction and repulsion subsisting between the atoms of gross matter and those of the electric matter and the æther of space. The primary forces determine the electric and æthereal atmospheres of molecules, originate the molecular forces proper, and also, when an inequality of electric condition is produced on two contiguous molecules or bodies by molecular actions, gives rise to the special forces of electric attraction and repulsion. The waves of heat and light originate in the æthereal atmospheres of molecules, and are developed by vibrations of the atoms of the electric atmospheres toward and from the centre of each molecule and the region of æthereal disturbance. By reason of the varying conditions of equilibrium, the rate of vibration increases, and its intensity or *vis viva* decreases, in proportion as the electric atom is more remote from the centre of the molecule. Thus of the different coloured rays the red proceeds from the lowest depth in the electro-æthereal atmosphere. The obscure heat-rays originate at a still lower depth. Heat and light may also originate in the space between two molecules in the act of combination, or near approach, by reason of the condensation of the interstitial electric æther toward the line of the centres, resulting from the oblique attractive action of the molecules. In this condensation of the electric æther between molecules that are urged nearer to each other, and the expansion of the same when they are separated, we find the key to the explanation of

the different modes of electric excitation (that of the galvanic current included). The secret of the intimate relations between electricity and heat and light is obvious in view of what has been stated.

The æthereal atmospheres of molecules, besides playing the part already signalized, are the chief determining cause of the diverse phenomena that attend the transmission of light through transparent media. Thus *refraction* is chiefly due to the retardation attending the propagation of the ray around from one side to the other of the molecular atmospheres ; *dispersion* of the rays in the spectrum to the fact that the rays of the greatest intensity and slowest rate of vibration penetrate to the greatest depth in the molecular atmospheres, pass around in smaller circles, and thus suffer the least retardation ; and *double refraction* to the fact that the atmospheres have a spheroidal form, owing to unequal molecular compression on different sides.

XV. *On the Temperature of Flames, and its relations with the Pressure. By* M. H. St.-Claire Deville[*].

IT is impossible not to be greatly struck by the numerous consequences which may be deduced from the experiments recently published by Professor Frankland, and of which he has given an account in an article in the *Comptes Rendus* of the 12th of last October[†]. I will ask leave from the Academy to develope here some ideas which this magnificent research has suggested to me, and to describe a plan of investigation commenced some time ago in my laboratory, the direction of which has been a little changed by the new facts discovered by the illustrious English chemist.

Professor Frankland (to sum up in brief his principal experiments) proves that the higher the pressure of an oxyhydrogen jet burning in a compressed atmosphere, the more brilliant and luminous[‡] does its flame become, which under the ordinary pressure is scarcely visible. At a high pressure a flame is obtained whose intensity may be compared to that of a wax candle. This single fact is sufficient to show the importance of such results, which may be said to have been as unforeseen as they are clearly and definitely established.

Professor Frankland finds the best explanation of this great

[*] From the *Comptes Rendus,* November 30, 1868.
[†] [See also Phil. Mag. for October, 1868, p. 309.]
[‡] To make a flame *brilliant*, it is sufficient that its rays, if they are simple and belonging to a monochromatic light, possess great intensity. That a flame shall be *luminous* in the ordinary acceptation of this word, it must possess almost all the rays of the solar spectrum ; it must be white, or as nearly so as possible by approximating to sunlight.

fact in the mere increase of density which necessarily accompanies the compression of the gas. He also draws conclusions which seem to invalidate the classical ideas introduced into science by Sir Humphry Davy, and which withdraw from the theory of flame a basis which has always appeared beyond the reach of attack. I confess that on this latter point I do not share Professor Frankland's ideas; and I base my opinion on certain facts as yet imperfectly investigated, but which I shall describe before long, when I shall have given them that demonstrative form which they want, and which in the present state of science must be given to all our speculations.

I shall not attack with the same firmness the questions relative to the influence of density on the luminous power of flames. I propose here to develope an idea the germ of which I find in the last paragraphs of Professor Frankland's communication. Our colleague ascribes the want of illuminating power in the flame of phosphorus burning in chlorine to the slight elevation of temperature which a combustion accompanied by so small a disengagement of heat must obviously produce. I believe this to be the real and only reason.

Let us first inquire what is the principal condition for luminosity in a flame. If we take an obscure but hot flame like that of a Bunsen's burner, and introduce common salt into it, every one knows that we obtain a light of feeble intensity and which is monochromatic; for the prism does not decompose it into a spectrum, and only produces one bright band. But if we increase the temperature of this flame (by adding oxygen, for instance), the lustre immediately revives, the number of lines increases, and hence approximates to a complete spectrum. The experiments of M. Fizeau and of MM. Wolf and Diacon are remarkably definite from this point of view. But suppose we use M. Debray's apparatus, by which, for spectroscopic experiments, a very high temperature (one of about $2500°$) can be obtained. In this flame the spectrum of sodium spreads out and becomes complete; it may be assumed, then, that the great number of brilliant lines which the spectrum contains merge into each other to form a whole which seems continuous. An observation of the same kind is made when large quantities of sodium are burnt in air or oxygen, or when lithium is set on fire: the flame of sodium, which is ordinarily yellow and monochromatic, that of lithium, which is usually red, both become white; they then contain all the rays or, we may say, all the brilliant lines of every refrangibility. They thus become luminous when the metal burns at a high temperature.

This observation is exact also for the invisible rays—for the chemical rays whose lines crowd and multiply in the spectrum in

proportion as luminous sources at higher temperatures are used to produce them. This is a cardinal observation due to M. Mascart. Thus the number of lines increases in the proportion in which the temperature rises in the flames which produce them ; and when the temperature attains a certain intensity, these lines merge into one another and give a continuous spectrum. The flame then necessarily becomes white, brilliant, and luminous.

A fact of the same kind is produced in Professor Frankland's experiment. The lines increase in number and in intensity in the hydrogen flame in proportion as the pressure on the explosive mixture within and without the blowpipe itself increases. What more rational conclusion can be drawn than that the temperature itself increases in the flame in proportion as the pressure increases? This is a prime fact, the demonstration of which may seem sufficient; but it is full of consequences so important that direct verifications should still be demanded. I shall revert subsequently to the consequences and the methods of verification which I think of using; but I desire to show at once that these considerations, deduced from spectrum-analysis, will explain the fact of the great illuminating power of arseniuretted hydrogen— a power that Davy's theory, which I think is incomplete from this point of view, can only explain by the supposed presence of a solid body in the flame. It is sufficiently evident that gases in burning give lines. If these lines are brilliant and numerous for reasons depending on the special nature of the substances observed, it is clear that the flame of these gases will be brilliant and the more luminous the greater the difference in the refrangibility of the lines these spectra contain. Here we have to do with a phenomenon belonging to arsenic in vapour contained in the flame of arseniuretted hydrogen ; and in the explanation of such a fact it seems to me useless to bring in the consideration of densities, invalidated moreover by the objection urged by Professor Frankland himself in the case of the flame of phosphorus burning in chlorine.

Thus the illuminating power of an entirely gaseous flame is a specific property connected with the production of lines furnished by the substances it contains ; it is as inexplicable as the specific properties of the bodies themselves—the density, colour, &c. Professor Frankland's idea, moreover, relative to the production in ordinary flames of very dense carburetted hydrogen seems to me difficult to rest on experiment. We know, in fact, that all these carburetted hydrogens decompose at the lowest temperatures into hydrogen and carbon—the latter hydrogenized, it is true, but opaque*. I think, then, that Davy's theory remains intact.

* I have proved (*Leçons sur la Dissociation*, p. 317, *Leçons de la Société Chimique*. Paris, Hachette, 1866) that in strongly heated carbonic oxide

I have said that if the flame of hydrogen becomes luminous under a high pressure, it arises from the temperature of the flame increasing in proportion as the pressure at which the combustion takes place itself increases. Let us now see what are the consequences of this fact, supposing it to be well established.

M. Debray and I have proved that the temperature of combination of hydrogen and oxygen, under the ordinary pressure, is 2500°. We determined this fixed point by throwing into water a kilogramme of melted platinum raised to the highest temperature which could be produced in a lime-furnace, taking into account the increase in the temperature of the water, the specific heat of platinum and the law of its increase given by M. Pouillet, along with its latent heat as determined by M. Person. We should have liked to control so important a result by a great number of tests, and to fix it, as far as the data of calculation permitted, in an incontestable manner. For that purpose it would have been necessary to use large masses of platinum, and to protect ourselves against very serious accidents, terrible explosions, of which we narrowly escaped being the victims. We were closely occupied with the solution of this question when Professor Bunsen published his beautiful memoir on the temperature of combustion*. The excellence of the method invented by the great Heidelberg physicist made it unnecessary for us to recur to a tedious and dangerous method, the more so as the numbers obtained by Professor Bunsen are in the most complete agreement with our own. Professor Bunsen gives 2800° as the temperature of combination of the two gases purified and introduced in a state of absolute dryness into his valve-eudiometer. Allowing for the moisture of the gases used in our experiments, and for the nitrogen brought into the gasholder by the water which displaced the gas, a number is obtained very near 2800°, which I shall adopt for the future as the true temperature corresponding to this phenomenon.

Taking the number 2500°, I obtained the fraction 0·44† to represent the portion of the gases which really combine at the

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dissociation took place with the production of oxygen and of a yellow pulverulent and light carbon, to which, according to all appearance, is due the blue tint of the flame. M. Cailletet has observed that, in withdrawing and suddenly cooling the gases from the tuyère of a blast-furnace by means of my hot and cold tubes, these gases, produced by a carbon absolutely destitute of volatile matters, were rendered almost opaque by a sort of thick brownish fog, which after the lapse of some time was resolved into a blackish-yellow deposit of extremely finely divided carbon.

* Pogg. *Ann.* vol. cxxxi. p. 161 ; Phil. Mag. S. 4. vol. xxxiv. p. 489.

† Compare *Leçons de la Société Chimique* (*de la Dissociation*), p. 290 (Paris, Hachette, 1866).

moment when (the heat of the mixture being a maximum) the dissociation of water corresponding to this temperature presents an obstacle to the complete union of its elements. Adopting the new number 2800°, we see that the part combined or not dissociated of the flame of hydrogen and of oxygen is really 0·50, or half the total mass.

Bunsen's valve-eudiometer enabled him to investigate the temperature of combustion when the total pressure of the oxygen and hydrogen is diminished and is brought below the atmospheric pressure. It is sufficient for this purpose if a certain quantity of an inert gas be added to the explosive mixture. Under these circumstances Professor Bunsen observed that this temperature rapidly decreased in proportion as the partial tension of the explosive gases was made to decrease. Consequently the quantity of matter dissociated, or the tension of dissociation of water in the flame, decreases with the temperature.

What would take place if we investigated the temperature of combination under a higher pressure than that of the atmosphere? This is obviously shown by Professor Frankland's experiments.

To acquire absolute certainty on this point a striking verification is required, which may be obtained either by melting platinum in an artificially condensed atmosphere, or by repeating Bunsen's experiments with the valve-eudiometer.

I am about to commence experiments of this kind ; they will be made in a laboratory with iron walls capable of resisting a pressure of at least three atmospheres—a pressure which the experiments made at the bridge of Kehl show is quite innocuous.

It is easy to understand the practical consequences which may flow from a series of experiments made under pressure with the ordinary combustibles. They lead to a direct trial of furnaces fed with air forced under a pressure equal to the pressure of the vapour in the generator. These furnaces, especially if they are fed with the mineral oils, the use of which is already beginning to be recommended, and which leave no residue—these boilers where the products of combustion compressed to five atmospheres, for instance, would move through the tubes with one-fifth the velocity of our present apparatus, would doubtless enable the surface of heating to be considerably diminished. It is owing to the interest that investigations of this kind may have in furnishing naval engineers with the data necessary for calculating the results, that the Emperor has been good enough to order that these experiments be made in the laboratory of the Ecole Normale. A large cylindrical chamber capable of holding the operator and his apparatus, and of supporting a considerable pressure of air furnished by a steam-pump, will form a laboratory

where all the manipulation necessary for determining the temperature produced by flames and solid combustibles may be effected without danger.

If, as is almost already demonstrated by what I have said and by almost all the observations made by engineers and by physicists in chambers containing compressed air, the temperature of combustion rises at the same time as the pressure increases, that would be one analogy more to be added to the number of those I have indicated between the phenomena of combination and of decomposition on the one hand, and the phenomena of the condensation of vapours and of volatilization on the other hand.

We may in fact give the name *greatest temperature of condensation of vapour* to what is improperly known as the *boiling-point of a liquid.* This temperature is no other than that commencing from which a vapour no longer condenses on the surface of a cold thermometer, which is merely heated by means of the latent heat yielded to it by the vapour in which it is immersed. The boiling-point, or temperature of condensation, rises, as we know, when the pressure above the liquid which produces the vapour is increased.

The combination of bodies, and particularly that of oxygen and hydrogen in the oxyhydrogen blowpipe, is apparently a more complex phenomenon, but corresponds perfectly to the act of the condensation of vapours.

Assuming that the temperature of the combination of hydrogen and oxygen is 2800°, the quantity of water formed under a pressure of 760 millims. will be in the flame, at the hottest part[*],

$$\frac{637 + (2800 - 100)\, 0\!\cdot\!475}{3833} = 0\!\cdot\!5 \; ;$$

that is to say, only half the oxygen and hydrogen will be combined under a pressure of 760 millims.

But if we increase the pressure, the temperature of the flame increasing also, it will be seen from the preceding formula that the proportion of substance combined or of aqueous vapour formed will increase as the pressure increases—just as the tension of a saturated vapour increases in proportion as the temperature increases. Lastly, the temperature of combination of a gaseous mixture, like the greatest temperature of condensation (or boiling-point) of a vapour, increases with the pressure.

The substance combined in a flame plays the same part as the substance condensed in a space full of vapour the temperature and pressure of which are varied so that the vapour is always saturated.

* Vide *Leçons de Chimie,* given in 1864 and 1865, p. 290 (Hachette, 1866).

It is clear from this that the quantity of substance uncombined or dissociated in the flame diminishes as the pressure increases. It may therefore be supposed that there is a pressure at which a mixture of hydrogen and oxygen would produce in combining the unimaginable temperature of 6800° which corresponds to total combination. But it is no more possible to make a serious hypothesis on this subject, than to ask whether there be a pressure at which water could no longer boil, whatever temperature were applied to it.

I hope the Academy will excuse my having so long dwelt upon a mere programme of researches in course of execution; but they will be long and tedious, and I have been anxious to preserve the right of pursuing them if any one more fortunate than myself should sooner reach the object I am desirous of attaining. If the general considerations developed in this communication should facilitate the solution of a problem which I propound for the first time, and which I seek by paths which, if complicated, are yet rational, I shall be happy to have prepared the way.

XVI. *On Ethylate of Sodium and Ethylate of Potassium.*—Part I. *By* J. ALFRED WANKLYN, *Professor of Chemistry in the London Institution*[*].

THE Ethylates of the Alkali-metals have been very imperfectly studied, and are well deserving of a minute investigation. Almost every one who has had occasion to prepare ethylate of sodium must have observed that the quantity of metal capable of being easily made to act on alcohol is comparatively small; from being very energetic, as it is at first, the action between the sodium and the alcohol soon becomes sluggish, and ceases long before so much as one equivalent of metal has decomposed one equivalent of alcohol. Nevertheless I believe that chemists usually regard the beautiful crystals which form when sodium is allowed to react upon alcohol as being ethylate of sodium, and as having the formula $C^2 H^5 O Na$.

The crystals are in reality a compound of ethylate of sodium with alcohol. A note by A. Geuther and E. Scheitz shows that they consist of $Na C^2 H^5 O, 2 (C^2 H^6 O)$. (I quote from the Chemical News of January 8, 1869, which quotes the note on Ethylate of Sodium from the Jena *Zeitschrift f. M. und N.* vol. iv. p. 16).

According to my own experiments, the crystals contain even more alcohol, viz. three molecules of alcohol to one molecule of ethylate of sodium, as will presently be described. It will also

appear that the absolute ethylates of the alkali-metals are endowed with an extraordinary degree of stability, being among the most permanent compounds belonging to organic chemistry.

Passing on to the description of my research :—A small glass retort, of 75 cubic centims. capacity (see figure), was cleaned, dried, and weighed. Into it was put some freshly cut sodium, the weight of which was ascertained. (Sodium admits of being accurately weighed, the thin film of oxide with which it so soon becomes covered being of insignificant weight.) Anhydrous alcohol was next poured into the retort, and the reaction between it and the sodium allowed to take place. The apparatus was then heated in the water-bath as long as any alcohol distilled over, and then cooled, dried, and weighed. The apparatus was a second time placed in the water-bath and subsequently cooled, dried, and weighed. The following are the numbers given by two experiments :—
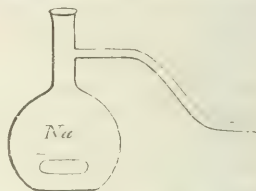
	I.	II.
Weight of sodium employed . . .	2·2 grm.	1·160
Quantity of absolute alcohol poured in, about 30 cub. centims.		
Weight of the solid product after heating to 100° C.	19·95	11·192
Weight of the same after second heating to 100° C.	19·75	

Calculated into percentage, we have—

Theory for $NaC^2H^5O, 3(C^2H^6O)$.			Found.	
			I.	II.
Na	. .	23	11·165	11·14	10·37
C^8	. .	96			
H^{23}	. .	23			
O^4	. .	64			
		206	100·00		

The crystals therefore consist of $Na\,C^2H^5O, 3(C^2H^6O)$, and will bear a temperature of 100° C. without losing alcohol. They are in a state of complete fusion at 100° C., and so long as the air is excluded remain quite colourless. A very slight exposure to the air tinges them with brown—a remark which is applicable to the absolute ethylate about to be described. They are not very soluble in ether; in a mixture of acetic ether and ether they appear to be more soluble.

On exposing them to temperatures above 100° C. these crystals give off alcohol, but they require a very considerable application of heat to drive off all the alcohol from them.

The 11·192 grms. of $Na\,C^2\,H^5\,O, 3\,(C^2\,H^6\,O)$ of experiment II. were gradually heated to 130° C. in the oil-bath, maintained at that temperature, and then raised to 140° C. The following are the numbers:—

	grms.
Weight of sodium employed	1·160
Weight of product at 100° C.	11·192
Weight of product at 140° C.	4·466

from which is deduced, percentage of sodium in product at 140°=25·98. The theory for absolute ethylate is, percentage of sodium = 33·82. It appears, therefore, that there was still about 23 per cent. of alcohol in the product; and from this circumstance it may be concluded that the retention of alcohol by ethylate of sodium is very obstinate.

On raising the temperature considerably higher the expulsion of alcohol is complete, and absolute ethylate remains behind, as is shown by the following:—

	grm.
Weight of sodium employed	0·584
Weight of product of action on alcohol after heating for some time from 190° to 205° C.	1·694

In a second experiment, in which the temperature of the oil-bath was high but not measured, the quantities were:—

	grm.
Sodium taken	0·498
Ethylate obtained	1·453

We have, therefore,

Exp. I. Percentage of sodium in product	=34·48
Exp. II. ,, ,, ,,	=34·28
Theory for $Na\,C^2\,H^5\,O$	=33·82

At about 200° C., therefore, pure absolute ethylate of sodium may be obtained from the crystals. This result was confirmed by an observation of the degree of alkalinity of the product which had been exposed to a temperature of 200° C. The 1·694 grm. which had been obtained from 0·584 grm. of sodium (Exp. I.) was placed in water, which of course resolved it into alcohol and caustic soda. It was then titrated with normal sulphuric acid. It saturated 25·5 cubic centims. of the acid, showing 0·5865 grm. of sodium in a state of causticity. All the sodium, therefore, was caustic; no organic acid, therefore, had been produced by the action of the high temperature on the ethylate of sodium. A still more severe test of the stability of the ethylate at 200° C. is afforded by the following experiment.

1·3285 grm. of sodium was dissolved in absolute alcohol, and

the resulting ethylate of sodium maintained for some time at about 200° C.   Weight of the product 3·862 grm.

$$\text{Found, Na per cent.} \quad . \quad . \quad = 34\cdot40$$
$$\text{Theory} \quad . \quad . \quad . \quad . \quad . \quad . \quad = 33\cdot82$$

The amount of alcohol given on distillation with water was next estimated.   Added 50 cubic centims. of water and distilled off 32·713 grms., which had a specific gravity of 0·9868 at 16° C., and which therefore consisted of 8·0 per cent. alcohol.   Weight of alcohol obtained =2·616 grms.   Therefore the quantity of alcohol yielded by 100 parts of the substance is 67·7 parts.   The theory for ethylate of sodium requires 67·65 parts.

Further experiment has also shown that even a temperature of 290° C. is sustained by ethylate of sodium without decomposition.

When ethylate of sodium is very strongly heated, viz. up to dull redness, having ceased to evolve alcohol, it evolves no appreciable quantity of liquid product, but carbonizes with evolution of a certain quantity of gas.   A rough examination of the gas showed the absence of olefines and of carbonic oxide.   The gas was of low specific gravity, and burnt with a not very luminous flame.   It was apparently marsh-gas, or marsh-gas and hydrogen.   The residual solid product contained (as was made out by titration) all the sodium in the state of metal, caustic, or carbonate ; therefore no stable acid had resulted.

The presence of free metallic sodium, or possibly carbide of sodium, in the residue was proved by the very violent effervescence which the addition of water to the residue occasioned. The presence of a considerable amount of carbonate was proved by the evolution of carbonic acid which took place on making the titration with standard acid.   A considerable quantity of black carbon separated on dissolving the product in water. Weighings were made of the amount of sodium which was converted into ethylate, and also of the total solid product left after the heating to redness, as follows :—

grms.

$$
\begin{aligned}
&\text{Sodium taken } = 0\cdot792 \text{ grm., equivalent to} \\
&\quad \text{to ethylate of sodium, } C^2 H^5 NaO \ . \quad .
\end{aligned}
\Biggr\} = 2\cdot342
$$

Residual solid product . . . . =1·588

Therefore volatile product . . . . . = ·754

A weighing was made of the black carbon got on treatment of the solid residue with water and test acid, and after subsequent washing and drying.
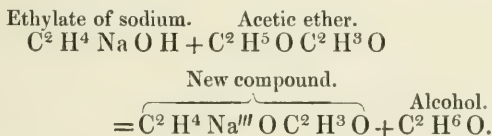
Black carbon =0·216 grm.

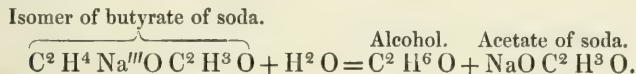(There had been a slight loss of the black carbonaceous matter, so that the number is only a rough approximation.)

Absolute ethylate of soda ($Na\,C^2\,H^5\,O$) is a perfectly white amorphous solid. It is non-fusible, differing totally from the crystals. Its specific gravity is very low; it swims in ether. It is almost insoluble in ether, 1000 parts of ether not dissolving more than 2 or 3 parts of the ethylate. As has been hinted, it becomes brown on exposure to the air; otherwise it may be heated to a very high temperature, 275° C. (and probably much higher), without losing its whiteness.

With acetic ether or valerianic ether it behaves in a very interesting manner. There is, first of all, combination, and afterwards, between 100° and 200° C., an abundant disengagement of alcohol.

$$\overset{\text{Ethylate of sodium.}}{C^2\,H^4\,Na\,O\,H} + \overset{\text{Acetic ether.}}{C^2\,H^5\,O\,C^2\,H^3\,O}$$

$$= \overset{\text{New compound.}}{\overbrace{C^2\,H^4\,Na'''\,O\,C^2\,H^3\,O}} + \overset{\text{Alcohol.}}{C^2\,H^6\,O.}$$

The new compounds (in the case of acetic ether an isomer of butyrate of soda, and in the case of valerianic ether an isomer of œnanthylate of soda) are being investigated. They appear to yield alcohol and a salt of soda on being treated with water, thus :—

$$\overset{\text{Isomer of butyrate of soda.}}{\overbrace{C^2\,H^4\,Na'''O\,C^2\,H^3\,O}} + H^2\,O = \overset{\text{Alcohol.}}{C^2\,H^6\,O} + \overset{\text{Acetate of soda.}}{NaO\,C^2\,H^3\,O.}$$

Potassium and Alcohol.

Potassium reacts with extreme violence on alcohol. It forms a compound consisting of ethylate of the metal and alcohol; the number of molecules of alcohol in combination with the ethylate has not been determined; but the existence of a compound of the kind is made out. At a high temperature, towards 200° C., the absolute ethylate is produced. During the expulsion of the alcohol there is a marked difference in appearance between the potash- and the soda-compound, the potash-compound remaining fusible at a much higher temperature than the soda-compound.

London Institution,
January 21, 1869.

XVII. *On the Relation of Hydrogen to Palladium.*
By Thomas Graham, *F.R.S., Master of the Mint*.*

IT has often been maintained on chemical grounds that hy-
drogen gas is the vapour of a highly volatile metal. The
idea forces itself upon the mind that palladium with its occluded
hydrogen is simply an alloy of this volatile metal, in which the
volatility of the one element is restrained by its union with the
other, and which owes its metallic aspect equally to both consti-
tuents. How far such a view is borne out by the properties of
the compound substance in question will appear by the following
examination of the properties of what, assuming its metallic cha-
racter, would have to be named *Hydrogenium.*

1. *Density.*—The density of palladium when charged with eight
or nine hundred times its volume of hydrogen gas is perceptibly
lowered; but the change cannot be measured accurately by the
ordinary method of immersion in water, owing to a continuous
evolution of minute hydrogen bubbles which appears to be de-
termined by contact with the liquid. However, the linear dimen-
sions of the charged palladium are altered so considerably that
the difference admits of easy measurement, and furnishes the
required density by calculation. Palladium in the form of wire
is readily charged with hydrogen by evolving that gas upon the
surface of the metal in a galvanometer containing dilute sulphuric
acid as usual†. The length of the wire before and after a charge
is found by stretching it on both occasions by the same moderate
weight, such as will not produce permanent distention, over the
surface of a flat graduated measure. The measure was graduated
to hundredths of an inch, and by means of a vernier the divi-
sions could be read to thousandths. The distance between two
fine cross lines marked upon the surface of the wire near each of
its extremities was observed.

Exp. 1.—The wire had been drawn from welded palladium, and
was hard and elastic. The diameter of the wire was 0·462 mil-
lim.; its specific gravity was 12·38, as determined with care.
The wire was twisted into a loop at each end and the mark made
near each loop. The loops were varnished so as to limit absorp-
tion of gas by the wire to the measured length between the two
marks. To straighten the wire, one loop was fixed, and the
other connected with a string passing over a pulley and loaded
with 1·5 kilogramme, a weight sufficient to straighten the wire
without occasioning any undue strain. The wire was charged
with hydrogen by making it the negative electrode of a small
Bunsen's battery consisting of two cells, each of half a litre in

* Read before the Royal Society January 7, 1869.
† Proceedings of the Royal Society, vol. xvi. p. 422 (1868). [Phil. Mag.
Ser. 4. vol. xxxvi. p. 63.]

capacity. The positive electrode was a thick platinum wire placed side by side with the palladium wire, and extending the whole length of the latter within a tall jar filled with dilute sulphuric acid. The palladium wire had, in consequence, hydrogen carried to its surface, for a period of $1\frac{1}{2}$ hour. A longer exposure was found not to add sensibly to the charge of hydrogen acquired by the wire. The wire was again measured and the increase in length noted. Finally the wire, being dried with a cloth, was divided at the marks, and the charged portion heated in a long narrow glass tube kept vacuous by a Sprengel aspirator. The whole occluded hydrogen was thus collected and measured; its volume is reduced by calculation to Barom. 760 millims., and Therm. 0° C.

The original length of the palladium wire exposed was 609·144 millims. (23·982 inches), and its weight 1·6832 grm. The wire received a charge of hydrogen amounting to 936 times its volume, measuring 128 cubic centims., and therefore weighing 0·01147 grm. When the gas was ultimately expelled, the loss as ascertained by direct weighing was 0·01164 grm. The charged wire measured 618·923 millims., showing an increase in length of 9·779 millims. (0·385 inch). The increase in linear dimensions is from 100 to 101·605, and in cubic capacity, assuming the expansion to be equal in all directions, from 100 to 104·908. Supposing the two metals united without any change of volume, the alloy may therefore be said to be composed of

				By volume.		
Palladium	.	.	.	100	or	95·32
Hydrogenium	.	.		4·908 or		4·68
				104·908		100

The expansion which the palladium undergoes appears enormous if viewed as a change of bulk in the metal only, due to any conceivable physical force, amounting as it does to sixteen times the dilatation of palladium when heated from 0° to 100° C. The density of the charged wire is reduced by calculation from 12·3 to 11·79. Again, as 100 is to 4·91, so the volume of the palladium, 0·1358 cubic centim., is to the volume of the hydrogenium 0·006714 cubic centim. Finally, dividing the weight of the hydrogenium, 0·01147 grm., by its volume in the alloy, 0·006714 cubic centim., we find

Density of hydrogenium . . . 1·708

The density of hydrogenium, then, appears to approach that of magnesium, 1·743, by this first experiment.

Further, the expulsion of hydrogen from the wire, however caused, is attended with an extraordinary contraction of the latter. On expelling the hydrogen by a moderate heat, the wire not only

receded to its original length, but fell as much below that zero as it had previously risen above it. The palladium wire first measuring 609·144 millims., and which increased 9·77 millims., was ultimately reduced to 599·444 millims., and contracted 9·7 millims. The wire is permanently shortened. The density of the palladium did not increase, but fell slightly at the same time, namely from 12·38 to 12·12; proving that this contraction of the wire is in length only. The result is the converse of extension by wire-drawing. The retraction of the wire is possibly due to an effect of wire-drawing in leaving the particles of metal in a state of unequal tension, a tension which is excessive in the direction of the length of the wire. The metallic particles would seem to become mobile, and to right themselves in proportion as the hydrogen escapes; and the wire contracts in length, expanding, as appears by its final density, in other directions at the same time.

A wire so charged with hydrogen, if rubbed with the powder of magnesia (to make the flame luminous), burns like a waxed thread when ignited in the flame of a lamp.

Exp. 2.—Another portion of the same palladium wire was charged with hydrogen in a similar manner. The results observed were as follows :—

Length of palladium wire . . . . . . 488·976 millims.
The same with 867·15 volumes of occluded gas 495·656 ,,
Linear elongation . . . . 6·68 ,,
Linear elongation on 100 . . . . . . 1·3663 ,,
Cubic expansion on 100 . . . . . . 4·154 ,,
Weight of palladium wire . . . . . . 1·0667 grm.
Volume of palladium wire . . . . . . 0·08072 cub. c.
Volume of occluded hydrogen gas . . . 75·2 ,,
Weight of same . . . . 0·00684 grm.
Volume of hydrogenium . . . . . . 0·003601 cub.c.

From these results is calculated

Density of hydrogenium . . . . 1·898.

Exp. 3.—The palladium wire was new, and on this occasion was well annealed before being charged with hydrogen. The wire was exposed at the negative pole for two hours, when it had ceased to elongate.

Length of palladium wire . . . . 556·185 millims.
Same with 888·303 volumes hydrogen 563·652 ,,
Linear elongation . . . . . . 7·467 ,,
Linear elongation on 100 . . . . 1·324 ,,
Cubic expansion on 100 . . . . 4·025 ,,
Weight of palladium wire . . . 1·1675 grm.

Volume of palladium wire	.	.	.	0·0949 cub. centim.
Volume of occluded hydrogen gas	.	84·3 cub. centims.
Weight of same .	.	.	.	.	.	.	0·007553 grm.
Volume of hydrogenium	.	.	.	.	0·003820 cub. centim·

These results give by calculation

Density of hydrogenium . . . . 1·977.

It was necessary to assume in this discussion that the two metals do not contract nor expand, but remain of their proper volume on uniting. Dr. Matthiessen has shown that in the formation of alloys generally the metals retain approximately their original densities *.

In the first experiment already described, probably the maximum absorption of gas by wire, amounting to 935·67 volumes, is attained. The palladium may be charged with any smaller proportion of hydrogen by shortening the time of exposure to the gas (329 volumes of hydrogen were taken up in twenty minutes), and an opportunity be gained of observing if the density of the hydrogenium remains constant, or if it varies with the proportion in which hydrogen enters the alloy. In the following statement, which includes the three experiments already reported, the essential points only are produced :—

TABLE.

Volumes of hydrogen occluded.	Linear expansion in millimetres.		Density of hydrogenium.
	From	To	
329	496·189	498·552	2·055
462	493·040	496·520	1·930
487	370·358	373·126	1·927
745	305·538	511·303	1·917
867	488·976	495·656	1·898
888	556·185	563·652	1·977
936	609·144	618·923	1·708

If the first and last experiments only are compared, it would appear that the hydrogenium becomes sensibly denser when the proportion of it is small, ranging from 1·708 to 2·055. But the last experiment of the Table is perhaps exceptional ; and all the others indicate considerable uniformity of density. The mean density of hydrogenium, according to the whole experiments, excluding that last referred to, is 1·951, or nearly 2. This uniformity is in favour of the method followed for estimating the density of hydrogenium.

On charging and discharging portions of the same palladium

* Philosophical Transactions, 1860, p. 177.

wire repeatedly, the curious retraction was found to continue, and seemed to be interminable. The following expansions, caused by variable charges of hydrogen, were followed on expelling the hydrogen by the retractions mentioned :—

		Elongation.				Retraction.
1st Experiment		9·77 millims.		.	.	9·70 millims.
2nd	,,	5·765	,,	.	.	6·20 ,,
3rd	,,	2·36	,,	.	.	3·14 ,,
4th	,,	3·482	,,	.	.	4·95 ,,
						23·99

The palladium wire, which originally measured 609·144 millims., has suffered, by four successive discharges of hydrogen from it, a permanent contraction of 23·99 millims. ; that is, a reduction of 3·9 per cent. on its original length. The contractions will be observed to exceed in amount the preceding elongations produced by the hydrogen, particularly when the charge of the latter is less considerable. With another portion of wire the contraction was carried to 15 per cent. of its length by the effect of repeated discharges. The specific gravity of the contracted wire was 12·12, no general condensation of the metal having taken place. The wire shrinks in length only. ·

In the preceding experiments the hydrogen was expelled by exposing the palladium placed within a glass tube to a moderate heat short of redness, and exhausting by means of a Sprengel tube ; but the gas was also withdrawn in another way, namely, by making the wire the positive electrode, and thereby evolving oxygen upon its surface. In such circumstances a slight film of oxide of palladium is formed on the wire, but it appears not to interfere with the extraction and oxidation of the hydrogen. The wire measured

					Difference.
Before charge	.	.	443·25 millims.		
With hydrogen	.	.	449·90	,,	+6·65 millims.
After discharge	.	.	437·31	,,	−5·94 ,,

The retraction of the wire therefore does not require the concurrence of a high temperature. This experiment further proved that a large charge of hydrogen may be removed in a complete manner by exposure to the positive pole (for four hours in this case) ; for the wire in its ultimate state gave no hydrogen on being heated *in vacuo*.

That particular wire which had been repeatedly charged with hydrogen, was once more exposed to a maximum charge, for the purpose of ascertaining whether or not its elongation under hydrogen might now be facilitated and become greater in consequence of the previous large retraction. No such extra elonga-

tion, however, was observed on charging the retracted wire more than once ; and the expansion continued to be in the usual pro- portion to the hydrogen absorbed. The final density of the wire was 12·18.

The wire retracted by heat is found to be altered in another way, which appears to indicate a molecular change. The metal gradually loses much of its power to take up hydrogen. The last wire, after it had already been operated upon six times, was again charged with hydrogen for two hours, and was found to occlude only 320 volumes of gas, and in a repetition of the ex- periment 330·5 volumes. The absorbent power of the palladium had therefore been reduced to about one-third of its maximum.

The condition of the retracted wire appeared, however, to be improved by raising its temperature to full redness by sending through it an electrical current from a battery. The absorption rose thereafter to 425 volumes of hydrogen, and in a second ex- periment to 422·5 volumes.

The wire becomes fissured longitudinally, acquires a thready structure, and is much disintegrated on repeatedly losing hy- drogen, particularly when the hydrogen has been extracted by electrolysis in an acid fluid. The palladium in the last case is dissolved by the acid to some extent. The metal appeared, how- ever, to recover its full power to absorb hydrogen, now conden- sing upwards of 900 volumes of gas.

The effect upon its length of simply annealing the palladium wire by exposure in a porcelain tube to a full red heat, was ob- served. The wire measured 556·075 millims. before, and 555·875 millims. after heating ; or a minute retraction of 0·2 millim. was indicated. In a second annealing experiment, with an equal length of new wire, no sensible change whatever of length could be discovered. There is no reason, then, to ascribe the retraction after hydrogen, in any degree, to the heat applied when the gas is expelled. Palladium wire is very slightly affected in physical properties by such annealing, retaining much of its first hardness and elasticity.

2. *Tenacity.*—A new palladium wire, similar to the last, of which 100 millims. weighed 0·1987 grm., was broken, in expe- riments made on two different portions of it, by a load of 10 and of 10·17 kilogrammes. Two other portions of the same wire, fully charged with hydrogen, were broken by 8·18 and by 8·27 kilogrammes. Hence we have—

> Tenacity of palladium wire . . . 100
> Tenacity of palladium and hydrogen 81·29

The tenacity of the palladium is reduced by the addition of hy- drogen, but not to any great extent. It is a question whether

the degree of tenacity that still remains is reconcilable with any other view than that the second element present possesses of itself a degree of tenacity such as is only found in metals.

3. *Electrical Conductivity.*—Mr. Becker, who is familiar with the practice of testing the capacity of wires for conducting electricity, submitted a palladium wire, before and after charging with hydrogen, to trial, in comparison with a wire of German silver of equal diameter and length, at $10°·5$. The conducting-power of the several wires was found as follows, being referred to pure copper as 100:—

Pure copper	100
Palladium	8·10
Alloy of 80 copper + 20 nickel .	6·63
Palladium + hydrogen . . .	5·99

A reduced conducting-power is generally observed in alloys; and the charged palladium wire falls 25 per cent. But the conducting-power remains still considerable, and the result may be construed to favour the metallic character of the second constituent of the wire. Dr. Matthiessen confirms these results.

4. *Magnetism.*—It is given by Faraday as the result of all his experiments, that palladium is "feebly but truly magnetic;" and this element he placed at the head of what are now called the paramagnetic metals. But the feeble magnetism of palladium did not extend to its salts. In repeating such experiments, a horseshoe electromagnet of soft iron, about 15 centims. (6 inches) in height, was made use of. It was capable of supporting 60 kilogs. when excited by four large Bunsen cells. This is an induced magnet of very moderate power. The instrument was placed with its poles directed upwards; and each of these was provided with a small square block of soft iron terminating laterally in a point, like a small anvil. The palladium under examination was suspended between these points in a stirrup of paper attached to three fibres of cocoon silk, 3 decimetres in length, and the whole was covered by a bellglass. A filament of glass was attached to the paper, and moved as an index on a circle of paper on the glass shade divided into degrees. The metal, which was an oblong fragment of electro-deposited palladium, about 8 millims. in length and 3 millims. in width, being at rest in an equatorial position (that is, with its ends averted from the poles of the electromagnet), the magnet was then charged by connecting it with the electrical battery. The palladium was deflected slightly from the equatorial line by $10°$ only, the magnetism acting against the torsion of the silk suspending thread. The same palladium charged with 604·6 volumes of hydrogen was deflected by the electromagnet through $48°$, when it set

itself at rest. The gas being afterwards extracted, and the palladium again placed equatorially between the poles, it was not deflected in the least perceptible degree. The addition of hydrogen adds manifestly, therefore, to the small natural magnetism of the palladium. To have some terms of comparison, the same little mass of electro-deposited palladium was steeped in a solution of nickel, of specific gravity 1·082, which is known to be magnetic. The deflection under the magnet was now 35°, or less than with hydrogen. The same palladium being afterwards washed and impregnated with a solution of protosulphate of iron of specific gravity 1·048, of which the metallic mass held 2·3 per cent. of its weight, the palladium gave a deflection of 50°, or nearly the same as with hydrogen. With a stronger solution of the same salt, of specific gravity 1·17, the deflection was 90°, and the palladium pointed axially.

Palladium in the form of wire or foil gave no deflection when placed in the same apparatus, of which the moderate sensitiveness was rather an advantage in present circumstances; but when afterwards charged with hydrogen, the palladium uniformly gave a sensible deflection of about 20°. A previous washing of the wire or foil with hydrochloric acid, to remove any possible traces of iron, did not modify this result. Palladium reduced from the cyanide and also precipitated by hypophosphorous acid, when placed in a small glass tube, was found to be not sensibly magnetic by our test; but it always acquired a sensible magnetism when charged with hydrogen.

It appears to follow that hydrogenium is magnetic, a property which is confined to metals and their compounds. This magnetism is not perceptible in hydrogen gas, which was placed both by Faraday and by M. E. Becquerel at the bottom of the list of diamagnetic substances. This gas is allowed to be upon the turning-point between the paramagnetic and diamagnetic classes. But magnetism is so liable to extinction under the influence of heat, that the magnetism of a metal may very possibly disappear entirely when it is fused or vaporized, as appears with hydrogen in the form of gas. As palladium stands high in the series of the paramagnetic metals, hydrogenium must be allowed to rise out of that class, and to take place in the strictly magnetic group, with iron, nickel, cobalt, chromium, and manganese.

Palladium with Hydrogen at a high Temperature.—The ready permeability of heated palladium by hydrogen gas would imply the retention of the latter element by the metal even at a bright red heat. The hydrogenium must in fact travel through the palladium by cementation, a molecular process which requires time. The first attempts to arrest hydrogen in its passage

through the red-hot metal were made by transmitting hydrogen gas through a metal tube of palladium with a vacuum outside, rapidly followed by a stream of carbonic acid, in which the metal was allowed to cool. When the metal was afterwards examined in the usual way, no hydrogen could be found in it. The short period of exposure to the carbonic acid seems to have been sufficient to dissipate the gas. But on heating palladium foil red-hot in a flame of hydrogen gas, and suddenly cooling the metal in water, a small portion of hydrogen was found locked up in the metal. A volume of metal amounting to 0·062 cubic centim., gave 0·080 cubic centim. of hydrogen ; or, the gas, measured cold, was 1·306 time the bulk of the metal. This measure of gas would amount to three or four times the volume of the metal at a red heat. Platinum treated in the same way appeared also to yield hydrogen, although the quantity was too small to be much relied upon, amounting only to 0·06 volume of the metal. The permeation of these metals by hydrogen appears therefore to depend on absorption, and not to require the assumption of anything like porosity in their stucture.

The highest velocity of permeation observed was in the experiment where four litres of hydrogen (3992 cubic centims.) per minute passed through a plate of palladium 1 millim. in thickness, and calculated for a square metre in surface, at a bright red heat, a little short of the melting-point of gold. This is a travelling movement of hydrogen through the substance of the metal with the velocity of 4 millims. per minute.

The *Chemical Properties* of hydrogenium also distinguish it from ordinary hydrogen. The palladium alloy precipitates mercury and calomel from a solution of the chloride of mercury without any disengagement of hydrogen ; that is, hydrogenium decomposes chloride of mercury, while hydrogen does not. This explains why M. Stanislaus Meunier failed to discover the occluded hydrogen of meteoric iron by dissolving the latter in a solution of chloride of mercury ; for the hydrogen would be consumed, like the iron itself, in precipitating mercury. Hydrogen (associated with palladium) unites with chlorine and iodine in the dark, reduces a persalt of iron to the state of protosalt, converts red prussiate of potash into yellow prussiate, and has considerable deoxidizing powers. It appears to be the active form of hydrogen, as ozone is of oxygen.

The general conclusions which appear to flow from this inquiry are that in palladium fully charged with hydrogen (as in the portion of palladium wire now submitted to the Royal Society) there exists a compound of palladium and hydrogen in a proportion which may approach to equal equivalents* ; that

* Proceedings of the Royal Society, 1868, vol. xvi. p. 425. [Phil. Mag. July 1868. p. 66.]

both substances are solid, metallic, and of a white aspect; that the alloy contains about 20 volumes of palladium united with 1 volume of hydrogenium, and that the density of the latter is about 2, a little higher than magnesium, to which hydrogenium may be supposed to bear some analogy; that hydrogenium has a certain amount of tenacity, and possesses the electrical conductivity of a metal; and, finally, that hydrogenium takes its place among magnetic metals. The latter fact may have its bearing upon the appearance of hydrogenium in meteoric iron, in association with certain other magnetic elements.

I cannot close this paper without taking the opportunity to return my best thanks to Mr. W. C. Roberts for his valuable co-operation throughout the investigation.

XVIII. *On some Phenomena of Binocular Vision. By* Joseph LeConte, *Professor of Chemistry and Geology in the University of South Carolina* *.

I. *Adjustments of the Eye.*

TWO kinds of ocular adjustment take place in every voluntary act of sight, viz. (1) a proper convergence of the optic axes so that they shall meet on the object of sight, and (2) an adjustment of each eye so that the diverging pencil of rays which enters the pupil shall be brought to perfect focus, and therefore produce a perfect image on the retina. The first or *binocular adjustment* is necessary for *single vision*; the second or focal adjustment is necessary for *distinct vision*. The first is distinctly sensible for all distances within 100 yards, and perhaps for much greater distances; the second is scarcely, if at all, sensible for distances beyond two yards.

To the two adjustments mentioned above may be added a third, viz. *contraction of the pupil*. The design of the contraction of the pupil is probably to increase the clearness of definition of the retinal image by cutting off the most divergent rays from very near objects, and thus to decrease the spherical aberration which is not entirely corrected in the eye by the form of the lens. The pupil, however, also contracts involuntarily under the stimulus of strong light, without regard to distance. This must be carefully distinguished from the adjustive contraction, which is (to some extent at least) voluntary.

These three adjustments of the eye, viz. binocular or axial adjustment, focal adjustment, and contraction of the pupil, are associated in every voluntary act of sight. They are accomplished by one act of volition. They are so intimately associated that they cannot be voluntarily separated. It is usually

impossible to converge the optic axes on any point without at the same time adjusting the lens and contracting the pupil in a manner suitable for perfect vision at that distance.   Such inseparably associated movements are called consensual movements.

The binocular adjustment is well understood ; there is no difference of opinion as to its necessity, nor the means by which it is accomplished.   But in regard to the focal adjustment, there has been much difference of opinion among the best physiologists and physicists.   Some have denied altogether the necessity, and therefore the existence, of any adjustment—attributing the phenomena which are usually explained by this means to mere transference of attention from near to distant objects, or *vice versâ*.   The large majority of the best physicists and physiologists, however, have for a long time regarded focal adjustment as an optical necessity, and therefore a fact ; but the real nature of this adjustment, and the means by which it is accomplished, has been a question in doubt.   It has been attributed by some to the elongation of the eye by the action of the recti muscles, by others to the change in the convexity of the cornea, by others to the structure of the crystalline lens combined with contraction of the pupil, by others to the pulling forward of the crystalline lens by the ciliary muscle so as to elongate the chamber behind the lens, and by still others to the change of form of the lens by the action of the ciliary muscle.   Recent very ingenious observations by Donders, Cramer, and Helmholtz upon the images of external objects made by reflection from the anterior surface of the crystalline lens, and the changes in form and size which they undergo when the eye is adjusted for near objects, have definitely settled the question in favour of a *change in the curvature of the lens*.   The mechanism by which this change is effected is not clearly known ; but it is probable that it is effected by the action of the ciliary muscle.

Before giving some experiments which bear upon the question of adjustment, I will state that my eyes are perfectly normal. In youth and early manhood the natural distance for distinct vision of small objects was eight inches ; but with effort I could see perfectly distinctly at five inches.   At the present time my natural distance for fine print is ten inches, though with effort I see distinctly at eight inches.   Beyond this there is for me no limit of distinct vision.   My eyes define the edge of the moon as perfectly as they do an object at the distance of ten inches. Moreover, by long practice I have acquired considerable, and perhaps very unusual facility in making experiments on binocular vision and in analyzing my visual impressions.   The following experiments, which I have practised from boyhood, are in-

teresting, not only as a beautiful illustration of the laws of binocular vision, but, I believe, as throwing some light on the subject of adjustment, and also upon the difficult subject of the *horopter*.

If a plane surface checkered or otherwise figured *in regular pattern*, such as an oil floor-cloth, a tessellated pavement, or a papered wall, be placed before the eyes at the distance of several feet and the optic axes be then voluntarily converged (the eyes crossed) upon some point in space nearer than the surface, the figures will of course be all seen double. If, now, the conver- gence be steadily increased until two contiguous similar images, one belonging to the right eye and one to the left, are made to coincide perfectly, and the eyes be then held steadily in this po- sition for some time, the patterned surface will be distinctly seen in exquisite miniature, not at its proper distance, but between the real object and the eye, at a distance depending upon the interval between the centres of the contiguous similar figures of the pattern. If the pattern be very regular, the illusion is com- plete; we actually seem to be looking at a real object. In this experiment the po- sition of the eyes is such that, of two contiguous si- milar figures, the right eye is directed toward the left figure and the left eye to- ward the right figure; and the image is seen at the crossing of the visual lines. Thus if one eye be directed toward *a* (fig. 1) and the other toward *b*, a perfect image of these two figures will be seen at *a'*. So also *b* and *c* will be united and seen at *c'* and *a* and *d* at *d'*, and so on for all the figures

Fig. 1.

of the pattern. The dotted line *d' a' c'* will be the position of the image-surface. The image thus obtained may be a little indi- stinct at first, but it gradually grows perfectly clear. As soon as the image is distinctly seen and the outlines of the figures well defined, it may be retained without any difficulty; for we seem to be looking at a real object, and therefore retain the ne- cessary convergence of the optic axes with ease. The eyes may now be turned in every direction, viewing this extensive image- surface precisely as if it were a real surface.
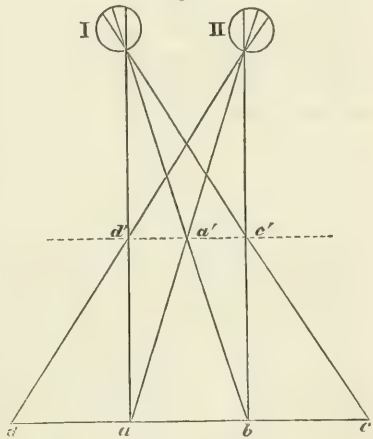
If, now, while viewing the image in the last experiment, we repeat upon it the same experiment, *i. e.* if by increasing the convergence of the optic axes we bring again the two contiguous figures into coincidence, a new image is formed between the last and the eye, and is seen in still smaller miniature.   In this case the position of the optic axes is such that the eyes crossing are directed, not toward contiguous figures of the real object, but to figures separated by an intervening one.   Thus in the figure (fig. 2) *a* and *c* will be combined and seen at a'', *d* and *b* at d'', and *b* and *e* at e''.

Upon this second image the same experiment may be repeated so as to make a third image still smaller and nearer the eye at d''', e''', and from the third even a fourth and still smaller image may be formed at e''''.   The positions of these successive planes are indicated by the dotted lines ; but in this figure the position of the axes is only adapted to vision on plane No. 2.   For the higher planes, the optic axes must converge still more.   For the fourth plane I *e* and I I *d* will represent the visual lines.   Standing erect

Fig. 2.
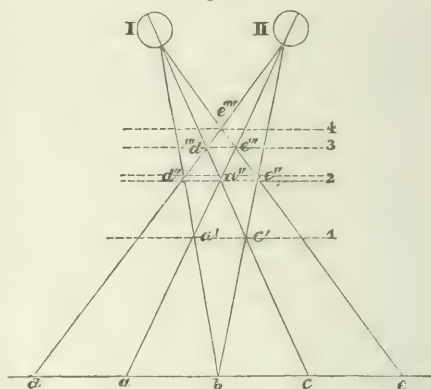
and looking down upon the regularly checkered carpet on the floor of my room, the figures of which are $4\frac{1}{2}$ inches from centre to centre, I can with the greatest ease bring out successively four distinct images one above the other, the nearest being but seven inches from my eyes, and the figures (which are 2 inches in diameter in the carpet) reduced to about $\frac{1}{4}$ inch in diameter. If while looking at the image on the fourth plane the convergence of the optic axes be suddenly relaxed, the image drops and may be caught on No. 3.   Again, by relaxing the convergence it may be dropped and caught on successive planes until it falls to its natural position.

I have made similar experiments on a great variety of patterns of wall-papering, oil-cloths, calicoes, &c. with the same results. Of a regularly checked oil-cloth in my hall, the lozenge-shaped figures of which are 10·2 inches across, I make successively three perfectly distinct images, the nearest being but $4\frac{1}{2}$ inches from the eyes.

Those who are not accustomed to experiments of this kind can probably most easily succeed as follows. If we look on the floor and place the finger between the eye and the floor, the finger will of course be seen double. Now move the finger up or down until we find a place in which the two images of the finger will exactly fall on contiguous figures of the pattern ; the finger now indicates the position of the first plane. Now look steadily at the finger instead of the floor until the image of the floor rises to it and becomes distinct, then withdraw the finger. To get the second plane, look again at the floor and raise the finger until its images fall upon figures separated from one another by an intervening figure and then look steadily at the finger. The other planes may be obtained in a similar manner. The position of the several planes may also be easily calculated—the data being the interocular line, the distance of the object, and the interval from centre to centre of the figures. Both by measurement and calculation I determined the planes in the case of the carpet to be 21·5, 13·05, 9·37, and 7·3 inches respectively. In the case of the oil-cloth they were 11·8, 6·54, and 4·5 inches.

If the distance between the centres of contiguous figures be less than the interocular line, then still other images may be seen *beyond* the real object and very much *enlarged*. The position of the eyes and the place of the image in this case also is easily explained. If *d a b c* (fig. 3) be the plane of the real

Fig. 3.
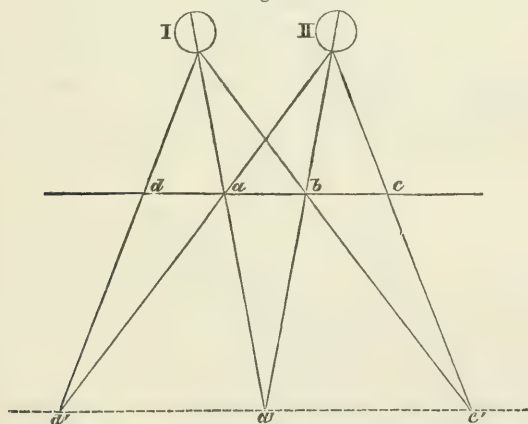
object, and the eyes I and II be directed toward contiguous figures *a* and *b* but not crossed, then the image of *a* and *b* will combine and be seen at *a′*, the intersection of the visual lines.

So also *a* and *d* will be seen at *d'*, and *b* and *c* at *c'*, and the dotted line will represent the position of the image-plane.   In order to make this image, we must gaze *through and beyond* the pattern until we observe the double images come together and coincide, and then fix the eyes steadily.   The enlarged image gradually becomes distinct.

This experiment is much more difficult than the preceding. The pattern should not be too small, otherwise the difficulty is very great.   In former years I had often performed the experiment with perfect success; but the wall-papering I had used for this purpose had been destroyed, and I found difficulty in again obtaining a suitable pattern.   I therefore constructed a pattern by ruling black lines on a large sheet of paper so as to make perfectly equal squares $1\frac{1}{2}$ inch wide.   With this simple diagram my success in all the preceding experiments was really marvellous.   The coloured patterns before used form far more beautiful images; but for scientific purposes the ruled diagram is far preferable.   With this diagram standing upright before me at the distance of sixteen inches, I got with great ease seven successive images on this side of the object, and one beyond.   All the images on this side were defined with great ease and *perfect distinctness*, although the nearest both by measurement and by calculation was but three inches from my eyes, *i. e.* far within the limits of my distinct vision.   With great effort I could obtain others still nearer.   The nearest I actually retained and measured was but $1\frac{1}{4}$ inch from the root of the nose; but I afterwards found that there was no limit except the root of the nose itself.   Within three inches, however, the images were no longer perfect, not from any want of distinctness of the lines, but because the horizontal lines of the two images were no longer parallel, but crossed one another, as shown in the figure (fig. 4), and therefore could not be made to coalesce perfectly.

Fig. 4.

The explanation of this will be given in its proper place. The still nearer images, as, for instance, those within $1\frac{1}{4}$ inch, could not be retained; the strain on the interior recti muscles of the eye was too great.

The image beyond the object is much more difficult to obtain with clearness, especially if the object be near the eyes.   At the

distance of two feet from the object I obtained the image very clearly and without much difficulty; but on approaching to within ten or twelve inches it was only by patient trial for some time that it could be brought out with perfect distinctness. When the object was twelve inches from the eyes, the image, by calculation, was found to be about thirty inches distant. By turning the diagram so that the diagonals were horizontal, and similar points therefore more than two inches apart, the image was seen at the distance of about six feet. It had the exact appearance of a tessellated marble pavement made up of squares nine inches on a side.

In all these experiments the least irregularity in the pattern shows itself very conspicuously in the image, not by indistinctness of outline of the figures, but by apparent inequality in the plane of the image. Thus in the carpet it shows itself by an apparent wrinkle, in the lined diagram by some of the lines rising like black threads stretched above the general surface of the image. This phenomenon is a familiar one in stereoscopy, and is used for detecting the slightest difference in two apparently similar patterns, as, for instance, between a genuine and a forged bank-bill.

I believe any one, and particularly any young person with good eyes, can with practice succeed in all the experiments detailed above. Several of my family have tried them with success. Yet in all cases it requires some practice to succeed well. I can, even yet, always detect some difficulty on first trial after an interval of a few days. But after several hours' practice the illusion is so complete that it is almost impossible to dispel it. The image is so real, that in attempting to recover the real object by relaxing the convergence of the optic axes, the doubling of the lines causes the eyes instinctively to return to their former position, and thus to restore the image. I have sometimes been actually obliged to look away in order to recover the real object.

———————————

The experiments detailed above have an important bearing on some points in the theory of vision. It is the universally accepted doctrine among physiologists that the axial and focal adjustments of the eye cannot be dissociated. Helmholtz, speaking of the consensual movements of the eyes, says, " We cannot turn one eye up and the other down; we cannot move both eyes at the same time outward; we are obliged to combine always a certain degree of accommodation of the eye to distance [focal adjustment], with a certain angle of convergence of the axes [axial adjustment]"*. He proceeds, however, to give cer-

* Helmholtz, Croonian Lecture, Proc. Roy. Soc. April 1864.

tain peculiar conditions under which the first two laws may be violated, but none in which the last is violated.   For many years I regarded these experiments as confirming the ordinary doctrine.   I had observed in my first experiments on the carpet that each successive plane became more and more indistinct. I accounted for this by supposing that both the optic axes and the lenses were adjusted for vision on the plane of the image, while the light diverged from the floor five feet distant.   It seemed to me a crucial experiment, proving the necessity of focal adjustment and the inseparable association of it with axial adjustment.   On recommencing these experiments a few weeks ago, however, I was struck with the fact that the figures of the images were far more distinct than the real figures were when a small object was viewed in the position of the images.   To test this point fairly, I placed two bone buttons in similar positions and on similar spots on the pattern, and then brought their images into coincidence.   At first the united image was indistinct, but gradually it became perfectly defined, every thread-hole as clear and distinct as it is possible to conceive.   I succeeded, though with greater difficulty, in getting a perfectly distinct image of the buttons on all the planes.   It was evident therefore that the indistinctness of the figures of the image on the higher planes was not the result of the want of focal adjustment, but of imperfection in the pattern.   The subsequent experiments with the ruled diagram proved this beyond the possibility of doubt.   The images in this case were obtained with much more ease, and the lines were defined with the most perfect sharpness, even when the image was brought nearly to the root of the nose.

In all cases, however, the image when first obtained was a little indistinct, and then gradually became clear.   With unpractised eyes this interval of indistinctness is considerable, but becomes shorter and shorter with practice, until it almost disappears.   When the image once becomes clear it remains so ; but there is then a sense, while looking at the image, of gazing beyond it ; or rather perhaps there is a difference between the image and the real object which we cannot account for, but which is not a difference of distinctness.   There is evidently an unnatural condition of the eyes, which produces strain and fatigue.

There is but one possible explanation of these phenomena, viz. that the *optic axes and the lenses are adjusted to entirely different distances*.   The three adjustments of the eye, viz. the axial adjustment, the focal adjustment, and the contraction of the pupil, have been so associated through successive generations, and the association so confirmed and strengthened in each

individual by constant practice from the earliest childhood, that a single act of volition accomplishes them all. Under ordinary circumstances they are so indissolubly associated that neither can be accomplished without the others. But the experiments described above prove that under certain circumstances *the first two* at least *may be completely dissociated*. In these experiments, when the image is first obtained, the optic axes, the lenses, and the pupil are all consensually adjusted for vision at the distance of the image ; and hence the image must be indistinct, for the rays diverge from an entirely different distance. But gradually the lenses adjust themselves to the actual divergence, *i. e.* for rays diverging from the real object, while the optic axes remain adjusted for the distance of the image. The difficulty experienced in dissociating these two adjustments causes the interval of indistinctness. The perception of the difference between the image and a real object is the sense of this dissociation. Consensual movements have been perhaps brought about by the necessities of single and distinct vision ; Helmholtz has shown* that other consensual movements may be dissociated when the necessities of single vision require it ; these experiments show that the consensual adjustments of the eye may be dissociated when the necessities of *distinct vision* require it.

I was now anxious to determine what part was taken by the pupil. Is the contraction of the pupil more intimately associated with the axial or the focal adjustment ? This question has been discussed by E. H. Weber, Cramer, and Donders†. Weber believes it is directly associated with the axial adjustment, Cramer and Donders with the focal adjustment. To test this question, while I was obtaining the image and making it clear, an assistant standing behind and a little to one side observed my pupil reflected in a small mirror conveniently placed. After gazing intently at the real object until the pupil was steady, as soon as I converged the optic axes so as to obtain the image No. 1, the pupil was observed to contract decidedly, but as the image became clear *it again expanded to its original size*. Again, at the moment of obtaining the second image the pupil contracted still more strongly, but as soon as the image became clear it again expanded nearly, if not entirely, to its original size. The same phenomena were observed for each of the images, only that in the nearest images, when the convergence of the optic axes was extreme and the first contraction very great, the pupil did not return entirely to its original dimensions.

I then made similar experiments on the image beyond the

* Proc. Roy. Soc. April 1864.

† Donders, "Accommodation and Refraction of the Eye," Transactions, p. 574.

real object.   As before, I looked intently first on the real object
at the distance of twelve inches until the pupil became steady.
So soon as I gazed beyond the object the pupil of course expanded ; but as soon as the image became clear, it *again contracted to nearly its original size.*   In this last experiment the
pupil is apt to be unsteady.   This might have been expected ;
for, as we have already said, it is much more difficult to obtain
this image clear, or to retain it when obtained.

There is no doubt of the fact, therefore, that *the contraction
of the pupil is most intimately associated with the focal adjustment.*

I believe that this principle of dissociation of consensual
adjustments explains perfectly certain phenomena of the stereoscope.   It is well known that many persons experience difficulty in seeing stereoscopic pictures distinctly even when the
two pictures are brought into perfect coincidence; and I believe all persons experience some fatigue to the eyes in looking at stereoscopic pictures for a considerable length of time.
I have often felt both the difficulty and the fatigue, though
to a much less degree than most persons.   The explanation
of this difficulty is as follows.   We judge of distance, as is
well known, by the axial adjustment.   If, then, the two pictures are so taken that, in order to bring them together, the
visual lines must meet at a certain distance, say, fifty yards,
then the picture will be seen at that distance, and of course very
much enlarged.   But in order to see the picture clearly, the
rays must come to the eye as if they diverged from the same
distance ; for the eyes are adjusted for that distance.   To fulfil
this condition lenses are always used ; but it is obvious that a
given pair of lenses are suitable for one distance only.   For all
other distances or degrees of optic convergence there must be
some degree of dissociation of the two adjustments ; and this is
both difficult and fatiguing to most persons.

I have found that observations upon the images of the ruled
diagram are a most delicate means of determining both the rotations of the eye and the position of the horopter.   I hope in my
next communication to take up this most difficult subject.

⌊To be continued.⌋

XIX. *Notices respecting New Books*.

Elementary Lessons in Astronomy. *By* J. Norman Lockyer, *F.R.A.S.*
London and Cambridge : Macmillan and Co.

THE schools of our country are much indebted to the author of this little work both for the substance and the shape of the information which he has given them.

The amount of thought which he has bestowed upon the arrangement of his materials has not been thrown away, but has produced a work which will give the young student (as well as children of a larger growth) a clear and more complete idea of that great whole called the universe than most works of greater pretensions.

The custom hitherto has been for writers on astronomy to direct their readers' attention rather to the instruments by which observations are made, and to the principles according to which they are discussed, than to invite them to begin by taking a bird's-eye view of the Cosmos. We are told how to adjust a transit, and how to measure the sun's distance from the earth ; but our energies are so much used up in understanding these things, that we have little strength left to contemplate as a whole the grand reality which they disclose.

But our author adopts a different method, and beginning with what we see, and first of all with the stars, we have a series of lessons in which the reader has clearly put before him a view of the magnitudes and distances of these bodies, as well as an account of their occasional peculiarities, such as colour and variability. In the nebular hypothesis, which is then described, we receive a hint of the process by which matter has been wrought from the state of primeval chaos into a sun or star.

When the reader has by this means become properly impressed with the magnitudes with which we deal in astronomy, one particular star is singled out for especial consideration. Our own star or sun is that one of all the host of heaven with which we are most intimately acquainted. His appearance and habits are therefore described, and we receive an insight into his chemical constitution.

Still proceeding downwards from greater to lesser magnitudes, we are next invited to consider the minor bodies of the solar system ; and just as the sun was singled out as the type of the stars, so the earth is singled out as the type of the planets. Astronomers have been fond of drawing attention to the adaptation implied in the fact that the gravitating centres of the various systems are also the centres of light and heat ; but it is only of late years that we have come to recognize that both these facts can be explained by the operation in two different ways of one and the same law. Accordingly we have another definition of planets, and one to which the author has given considerable prominence, namely that planets are cold while suns are hot, just as truly as that they are wandering while suns are fixed.

In clearness of diction, in comprehensiveness, in beauty of illustration this little volume is all that can be desired. In Lesson XLIV. we have an extremely useful summary of the methods by which the true positions of the heavenly bodies are obtained; and at the end of the volume we have a very complete and withal extremely intelligible account of the law of gravitation.

XX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 75.]

November 19, 1868.—Lieut.-General Sabine, President, in the Chair.

THE following communications were read :—

"On the Lightning Spectrum." By Lieut. John Herschel, R.E.

I have had two or three opportunities of seeing this spectrum to advantage of late. The storms at the period of the setting in of the south-west monsoon here are very frequent, and supply for a time almost incessant flashes, many of which are of course very brilliant. The first time I examined the light in the spectroscope I had no idea of measuring, but was content to realize the principal facts of a continuous spectrum crossed by bright lines; but subsequently I made several attempts (with some success) to obtain measures. That I was unable to do more in this line is due partly to the difficulty of utilizing the short-lived appearance, partly to that fascination of waiting for " one more " bright flash to verify the intersection, which can only be thoroughly appreciated by the aid of a similar experience.

The principal features of the spectrum are a more or less bright continuous spectrum crossed by numerous bright lines, so numerous indeed as to perplex one as to their identity. This perplexity is increased by the constantly changing appearance due to a variable illuminating-power. This variable character of the appearances is unquestionably the peculiar feature of the spectrum. It is not that the whole spectrum varies in brightness in the same degree, but that the *relative* intensities are variable, not only among the various lines, but between these and the continuous spectrum. The latter is sometimes very brilliant; and when that is the case, the red portion is very striking, though in general the spectrum seems to end abruptly at $D+0.34$ ($E=D+1.38$, Kirchhoff's $120.7=D+0.55$).

There is one principal line which I found equal to $D+2.20$ as the result of five independent measures. The probable error of this value is about $\pm .02$. The general mean of all my measures of the principal *nebular* line (obtained from twelve different nebulæ) is 2.18, with a probable error of about $\pm .02$. I have therefore very little doubt that these are the same, viz. the nitrogen line identified in the case of nebulæ by Mr. Huggins. This line in the lightning spectrum is narrow and sharply defined, and is conspicuously the brightest, except as noted below.

The next in prominence is situated about $D + 3.58$ ($F = D + 2.73$, Kirchhoff's $232.5 = D + 3.50$). It is broader and less vivid, and not so well defined at the edges.

There are several other conspicuous lines, but none comparable to the first. I noticed a sharp line in the red, but did not get a measure.

I said that at $D + 0.34$ the continuous spectrum ends abruptly. A faint continuation, however, is frequently seen in bright flashes, very bright ones bringing out a brilliant red end crossed by a bright line.

The whole of the ordinary spectrum *seems* green and blue, or rather greenish blue; but as the usual prismatic order of colours is recognizable in bright flashes, it is to be inferred that the region from E to F is so much brighter as to give the character in question. What strikes one most, however, is the varying relative brightness of the continuous and linear spectra; sometimes the lines are scarcely seen, and sometimes very little else is seen. This may be nothing more than an illusion; but in the absence of any certainty that it is so, the impression left on the mind is worth recording.

The difficulty of discriminating between the many less prominent lines is immensely increased by the momentary character of the phenomenon. Before the mind has selected an individual, the feeble impression on the retina has vanished; and before another flash succeeds, the memory of the half-formed choice has vanished with it, and there is nothing on which to found a selection. Otherwise it would be easy enough to measure many more lines.

"Notice of an Observation of the Spectrum of a Solar Prominence." By J. N. Lockyer, Esq., in a Letter to the Secretary.

<div align="right">October 20, 1868.</div>

Sir,—I beg to anticipate a more detailed communication by informing you that, after a number of failures, which made the attempt seem hopeless, I have this morning perfectly succeeded in obtaining and observing part of the spectrum of a solar prominence.

As a result I have established the existence of three bright lines in the following positions :—

    I. Absolutely coincident with C.

   II. Nearly coincident with F.

   III. Near D.

The third line (the one near D) is more refrangible than the more refrangible of the two darkest lines, by eight or nine degrees of Kirchhoff's scale. I cannot speak with exactness, as this part of the spectrum requires remapping.

I have evidence that the prominence was a very fine one.

The instrument employed is the solar spectroscope, the funds for the construction of which were supplied by the Government-Grant Committee. It is to be regretted that its construction has been so long delayed.

<div align="center">I have, &c.,</div>

<div align="right">J. Norman Lockyer.</div>

The Secretary of the Royal Society.

Supplementary Note.   Received Nov. 5, 1868.

Sir,—I have the honour, in continuation of my letter of the 20th ultimo, to inform you that I have this morning obtained evidence that the solar prominences are merely the expansion, in certain regions, of an envelope which surrounds the sun on all sides.   I may add that other facts observed seem to point out that we may shortly be in a position to determine the temperature of these circumsolar regions.

J. Norman Lockyer.

"Spectroscopic Observations of the Sun."—No. II.   By J. Norman Lockyer, F.R.A.S. (This paper was concluded on November 26).

The author, after referring to his ineffectual attempts since 1866 to observe the spectrum of the prominences with an instrument of small dispersive powers, gave an account of the delays which had impeded the construction of a larger one (the funds for which were supplied by the Government-Grant Committee early in 1867), in order that the coincidence in time between his results and those obtained by the Indian observers might not be misinterpreted.

Details are given of the observations made by the new instrument, which was received incomplete on the 16th of October. These observations include the discovery, and exact determination of the lines, of the prominence-spectrum on the 20th of October, and of the fact that the prominences are merely local aggregations of a gaseous medium which entirely envelopes the sun. The term *Chromosphere* is suggested for this envelope, in order to distinguish it from the cool absorbing atmosphere on the one hand, and from the white light-giving photosphere on the other. The possibility of variations in the thickness of this envelope is suggested, and the phenomena presented by the star in Corona are referred to.

It is stated that, under proper instrumental and atmospheric conditions, the spectrum of the chromosphere is always visible in every part of the sun's periphery ; its height, and the dimensions and shapes of several prominences, observed at different times, are given in the paper. One prominence, 3′ high, was observed on the 20th October.

Two of the lines correspond with Fraunhofer's C and F ; another lies 8° or 9° (of Kirchhoff's scale) from D towards E. There is another bright line, which occasionally makes its appearance near C, but slightly less refrangible than that line. It is remarked that the line near D has no corresponding line ordinarily visible in the solar spectrum. The author has been led by his observations to ascribe great variation of brilliancy to the lines. On the 5th of November a prominence was observed in which the action was evidently very intense ; and on this occasion the light and colour of the line at F were most vivid. This was not observed all along the line visible in the field of view of the instrument, but only at certain parts of the line, which appeared to widen out.

The author points out that the line F invariably expands (that the

band of light gets wider and wider) as the sun is approached, and that the C line and the D line do not; and he enlarges upon the importance of this fact, taken in connexion with the researches of Plücker, Hittorf, and Frankland on the spectrum of hydrogen— stating at the same time that he is engaged in researches on gaseous spectra which, it is possible, will enable us to determine the temperature and pressure at the surfaces of the chromosphere, and to give a full explanation of the various colours of the prominences which have been observed at different times.

The paper also refers to certain bright regions in the solar spectrum itself.

Evidence is adduced to show that possibly a chromosphere is, under certain conditions, a regular part of star-economy ; and the outburst of the star in Corona is especially dwelt upon.

GEOLOGICAL SOCIETY.

May 20th, 1868.—Prof. T. H. Huxley, LL.D., F.R.S., &c., President, in the Chair.

The following communications were read :—
1. " On the Eruption of the Kaimeni of Santorin." By Dr. J. Schmidt.

The eruption to which this paper referred commenced in January 1866, and continued uninterruptedly up to the close of the year 1867. Probably years may elapse before the volcanic energy has died out.

The eruption of the Nea-Kaimeni originated on the south side of the island, and extended towards the west. The tendency of the lava-current was southwards, and the extension, after about two years was from 1200 to 1400 yards southward, and 1800 yards from east to west. On account of the great depth of the water and the continual access of the open sea, the temperature of the water has not been remarkably elevated, varying from 77° to 122° F. The old George harbour has been greatly improved by the upheaval of the southern and western sides, while the channel between Nea and Micra Kaimeni has been shallowed, so as to be passable only for boats.

The author then described the George volcano, and stated that an eruption of stones and ashes, accompanied generally with sharp explosions, took place about every seven minutes. Immediately after these stone-showers hissing columns of white steam succeeded, and these were followed by faint-yellow noiseless issues from the central fumarole. None of the stones were thrown more than 400 feet above the water. It is impossible to predict anything with regard to the cessation of the eruption, although it has diminished in intensity since 1866.

Capt. SPRATT pointed out that this was only one of the many peaks in the large crater of Santorin which have risen up since the historical period. In the position in which he had anchored but six or seven years ago there is now a hill upwards of 300 feet in height.

Sir RODERICK MURCHISON referred to the communications to the

French Academy relative to the chemical products of the eruption, and their relation to those of Vesuvius and other volcanoes.

Mr. Forbes directed attention to the fact alluded to in the late President's anniversary address, that the lavas of this volcanic out-burst were, at its commencement, trachytes, or of highly silicated character, but afterwards were basic lavas—thus proving that rocks of totally different characters and chemical composition (respec-tively analogous to the granitic and trappean rocks of former periods) might proceed from a volcanic focus during an eruption.

Prof. Ansted called attention to the probable connexion of the eruptions in these islands with those of Vesuvius and Etna, and mentioned that Baron von Waltershausen had presented to the Society photographs of his magnificent original drawings of the whole region of Etna, which were upon the table, of which only three copies were taken on a larger scale than the published maps.

2. " On the Structure of the Crag-beds of Norfolk and Suffolk, with some observations on their Organic remains.—Part II. The Red Crag of Suffolk." By Joseph Prestwich, Esq., F.R.S., F.G.S.

The superposition of the Red Crag to the Coralline having been clearly shown by previous writers, the author confined his paper to those questions on which differences of opinion still exist, namely, the structure of the Red Crag, its affinities with the Coralline, and its exact relation to the Mammaliferous Crag of Norfolk. The Red Crag of Suffolk was described as occupying an excavated area in the Coralline, wrapping round the isolated reefs of the latter, filling up the hollows between them, and occupying a similar, and sometimes a rather lower level than the summits of these older reefs. It forms such an extremely variable series of beds, that the author had been unable to observe any definite order of succession in the greater part of it ; but he remarked that oblique lamination is most strongly developed in the lower and central portions, and that almost everywhere there occurs at the base a bed of phosphatic nodules, although deposits of that nature are by no means confined to one level. Old sea-cliffs of Coralline Crag, and remains of old sea-beaches at their base, were described by Mr. Prestwich as oc-curring at Sutton ; and he also gave detailed descriptions of nume-rous pits in the Red Crag of Suffolk, where the phenomena which he described may be observed. Dividing the Red Crag into an upper, frequently unfossiliferous, member (the fossils of which, being most frequently in the position in which they lived, may be re-garded as truly representing the fauna of the period), and a lower fossiliferous portion (in which the shells are found mostly in a broken and comminuted state and mixed largely with fossils derived from the older Coralline Crag), the author described their distribution in Suffolk, and their mode of occurrence on the eroded Coralline Crag, referring more especially to the difficulty in drawing the line be-tween them in many cases.

In treating of the Organic Remains of the Red Crag, Mr. Prest-wich gave lists of the shells found at the different localities, which had been prepared with the aid of Mr. Gwyn Jeffreys. Taking

the local conditions into consideration, eliminating the extraneous fossils of the Red Crag of Sutton, Butley, &c., and excluding the freshwater fossils of the more northern districts, the author regarded the remaining fossils of the two divisions of the Red Crag as being so closely related that the whole group must palæontologically be treated as one. Mr. Searles Wood had given the total number of species of its mollusca as 239 ; to these Mr. Gwyn Jeffreys has added six additional species ; on the other hand, he regarded ninety-nine of them as varieties and extraneous fossils, leaving 146 species belonging to the Red Crag. Of these Mr. Jeffreys has identified 133, or 92 per cent., with living species, 115 still being inhabitants of British seas, 15 being found in more northern seas, and 3 in more southern.

From the Mammaliferous Crag of Norfolk and the Red Crag of Suffolk never having been found in superposition, from the circumstance that just at the point where the latter ceases the former begins, as well as from the community of so many species of organic remains, the author regarded the two deposits as equivalent ; and he attributed their distinctive characters partly to the extraneous fossils in the Red Crag, and partly to the difference in the conditions which prevailed in the two areas at that time, and especially to the more littoral and brackish-water conditions which prevailed in the Norfolk area. In conclusion, Mr. Prestwich gave a sketch of the physical history of the Red-Crag period, describing the mode in which the various phenomena he had noticed had been produced.

The Rev. Mr. GUNN, in opposition to the view of the Forest-bed being placed above the Chillesford clay, mentioned that at Easton Bavent, where the latter has been supposed to occur in the cliff, he had seen the Forest-bed exposed on the shore. He instanced other cases where the Forest-bed, in his opinion, underlay the Chillesford clay and sands, and supported his views by the evidence of the Mammalian remains of the different beds, and especially the succession of the *Mastodon Arvernensis,* the *Elephas meridionalis, E. antiquus,* and *E. primigenius.* He regretted the absence of any mention of the Mammals of the Red Crag.

Mr. GWYN JEFFREYS made some remarks on the subject of species, and explained how, from a comparison of a large number of specimens, he had in many instances been led to reduce what had formerly been considered distinct species into mere varieties of the same species. He corroborated the views of the author as to the presence in the Red Crag of numerous fossils of the Coralline Crag.

Dr. COBBOLD stated that, from a microscopic examination of the phosphatic nodules, he had established the existence in them of Radiolariæ and Diatomaceæ, and especially of *Arachnoidiscus cocconeis,* the Radiolariæ being chiefly of the division Acanthometræ, all three forms being purely marine.

Mr. CHARLESWORTH commented on the remarkable fact that in a few thousand square feet of Coralline Crag we have a fauna as extensive as the whole British molluscan fauna. He considered that at present the attempt to solve the question of the age of the Red

Crag was hopeless, mainly from the difficulty of recognizing extraneous fossils. He expressed his disappointment at the fish-fauna of the Red Crag not having been noticed by the author. The teeth which were common to the Eocene and Red Crag had usually some phosphatic matter adherent. Those, on the contrary, which only occur in the Crag, have never any phosphatic matter attached. He therefore regarded the former class as derivative, but the latter as belonging to the deposit in which they occur.

Mr. SEARLES WOOD, Jun., denied that the Red Crag was the one homogeneous deposit divided into two beds represented by Mr. Prestwich; he protested against the Walton and Butley deposits being regarded as one and the same—the former bearing more affinity to the Coralline Crag, and being therefore probably the older.

Mr. PRESTWICH, in reply, explained that he did not intend to omit the lists of mammalian remains of the Red Crag, Tables of which were appended to the paper, the greater part of them, however, he regarded as derivative. With regard to the relation of the Chillesford beds to the Forest-bed, he had never *seen* a section in which the latter underlay the former; the Chillesford beds at Easton Bavent were underlain by sandy beds referable to the Norwich Crag. He considered that some division in the lower bed, as suggested by Mr. Searles Wood, was to be found.

June 3rd, 1868.—Prof. T. H. Huxley, LL.D., F.R.S., &c., President, in the Chair.

The following communications were read :—

1. " On some Genera of Carboniferous Corals." By James Thomson, Esq.

Mr. Thomson gave a *résumé* of the diagnostic peculiarities of *Cyathophyllum*, Goldfuss, *Clisiophyllum*, Dana, *Aulophyllum*, Milne-Edwards and Jules Haime, and *Cyclophyllum*, Duncan and Thomson. The author then noticed that the separation of these genera was inevitable and necessary, from the ordinary rules of the classification of the Zoantharia. He concluded by remarking upon the evident structural distinctions between *Clisiophyllum*, *Aulophyllum*, and *Cyclophyllum*.

Dr. DUNCAN said that the existence of a columella was a generic distinction in recent and mesozoic corals, that the type of the palæozoic Cyathophyllidæ was reflected in the Lower-Liassic coral-fauna of South Wales and the west of England, and that there was a necessity for the same principles of classification in the palæozoic and in the recent coral-fauna. There was a gradation from the Rugosa to the Aporosa.

Prof. HUXLEY remarked that the structure of the specimens of the different genera proved that there were great difficulties in accepting Agassiz's opinion that these old forms were not Zoantharia.

2. " On the Pebble-beds of Middlesex, Essex, and Herts." By S. V. Wood, Jun., Esq., F.G.S.

The author described some pebble-beds in the counties of Middlesex, Hertford, and Essex, which, in the MS. memoir deposited

in the Society's Library, he had described and referred to the age of the Bagshot sands. Premising that there could be no mistake as to the beds under discussion, as Mr. Hughes and the author had found their lines of the Middle Glacial gravel (termed "gravel of the lower plain" by the former geologist) to coincide, Mr. Wood described the distribution of the gravels which he regarded as of Bagshot age, and gave an account of the physical phenomena which he conceived had led to their deposition and subsequent denudation. He objected to Mr. Hughes's view, that the pebble-bed termed "gravel of the higher plain" by Mr. Hughes belongs to a period anterior to the gravel of the lower plain, as it involves the admission that there has been a period intermediate between that of the Bagshot pebble-beds and the Glacial, during which the sea occupied these counties and deposited the gravel of the higher plain—an admission which would be fatal to his view of the Bagshot age of the pebble-beds described in this paper.

Mr. PRESTWICH was inclined to regard some of the beds referred by the author to the Bagshot series rather as local drifts derived mainly from those beds than as the beds themselves.

Mr. WHITAKER saw a difficulty in classing the pebble-beds at Brentwood and elsewhere among the Bagshot beds, as in the London district, at all events, no such pebble-beds occur in the Bagshot series.

Mr. EVANS pointed out the difficulty in supposing that the gravels at the high level could have been deposited at a later period than those of the low level without, at the same time, overlying the latter.

Mr. SEARLES WOOD considered that there was not that broad line of distinction to be drawn between the gravels of the higher and lower level; he maintained that the pebble-beds when truly *in situ* were free from Quartzite, and truly of the Bagshot age.

3. "On the Cretaceous Rocks of the Bas-Boulonnais." By William Topley, Esq., F.G.S., of the Geological Survey of England and Wales.

After a *résumé* of previous notices on the subject, the author described the Physical Geography of the district and the Cretaceous beds below the chalk, comparing them with their English equivalents.

Each great division of the Kentish series was stated to be represented in the Boulonnais, although, in every case, in diminished thickness. The Upper Greensand and Gault were shown to surround the district at the base of the chalk hills; and a fossiliferous phosphatic bed was described at the bottom of the Gault, as in Kent. This bed was regarded by the author as a passage between Gault and Lower Greensand, as nodules with fossils often occur in the sands below; and it was shown to be frequently impossible in sections to mark off accurately the Lower Greensand from the Gault. The marked change in the faunæ of these formations was regarded by the author as due to the complete change in the conditions of deposit.

The sands which occur below the Gault were shown to belong

partly to the Folkestone beds (or highest division of the Lower Green-
sand) and partly to the Wealden—the intermediate stages being
absent, although well developed where last seen on the Kentish
coast.   The ferruginous sands, with variegated clays and iron-ore,
which cap the hills in the interior of the Bas-Boulonnais, were re-
ferred by the author to the Wealden series, as were also the pebble-
beds of St. Etienne and elsewhere, hitherto regarded as " drift."

The Wealden beds were shown to rest upon the Portland around
Boulogne, and upon lower members of the Oolites in the west and
north ; while in the north-west corner they fill " pipes " in Palæo-
zoic limestones.   The Wealden beds, thus proved to be unconform-
able to those below them, were shown to underlie conformably the
remaining Cretaceous beds above, thinning away, however, against
the old ridge, where, by overlapping the Lower Greensand and finally
the Gault, they rest immediately upon the Palæozoic rocks.

The paper was illustrated by a map, showing the probable out-
crop of the Cretaceous rocks beneath the English Channel.

Sir RODERICK MURCHISON, without doubting the correctness of
the author's views, wished that fossil evidence had been forthcoming
to identify more conclusively the Wealden strata of the Boulonnais
with those of England, and suggested their correlation with the
Beauvais beds.

The Rev. Mr. WILTSHIRE remarked that in Kent the *Ammonites
mammillaris* was contained in large nodules, and occurred only
below the lower phosphatic band.

Mr. WHITAKER, who had been with the author in the Boulonnais,
had been, contrary to his predilections, compelled to regard the beds
referred to the Wealden as belonging to that formation, and not to
the Lower Greensand.

4. " Note on the Mendip Anticlinal." By C. H. Weston, Esq.,
F.G.S.

The author called attention to the discovery of igneous rocks in
the north-western portion of the Mendip Hills long previous to Mr.
Moore's discovery of them in the south-east ; and he stated that this
fact left no doubt about the persistence of this upheaving agent
throughout the entire anticlinal of the Old Red and Carboniferous
series.

June 17th, 1868.—Prof. T. H. Huxley, LL.D., F.R.S.,
President, in the Chair.

The following communications were read :—
1. " On the Distribution of Stone Implements in Southern India."
By R. Bruce Foote, Esq., F.G.S., of the Geological Survey of India.

The chipped stone implements of Southern India are found in,
or associated with, two formations—the coast-laterite, which is a
marine formation, and a freshwater deposit, occurring inland at
greater elevations above the sea.   Most of them have been found
either *in situ* in the laterite of the eastern coast, or distributed over
its surface ; several have been collected off the surface of older
rocks, in places where the laterite had been removed by denudation ;
others have been discovered on the surface at great elevations in

other parts of the country, where no distinct traces could be seen of the formation from which they had weathered out, and which had a different origin (possibly freshwater) from that of the marine coast-laterite; while a few have been obtained from unquestionably fluviatile deposits. None have been collected from formations known to be either younger or older than the coast-laterite.

The author inferred that during the latter part of the laterite-period the land was raised to the extent of 500 or 600 feet, that this elevation was followed by a period of quiescence during which the laterite was extensively denuded, that this epoch was succeeded by a period of depression during which the recent coast-alluvium was formed, and that a subsequent elevation brought the land into its present position.

The President referred to the evidence of physical geography to prove that the Deccan was once an island, and to ethnological data to prove that the people who made the quartzite implements were probably not the original Aryans, but were the ancestors of the Hill tribes, whose nearest affinities are with the aboriginal Australians of the present day. He was of opinion that the two populations were once nearly or quite continuous, having been subsequently cut into segments by geological changes, and that the makers of the quartzite implements came from the same stock as both these recent tribes, which present the most rudimentary civilization known.

Prof. Rupert Jones called attention to the similarity in the type of these quartzite implements to that of the flint implements of Europe.

Sir Roderick Murchison doubted whether the laterite was a marine formation, as neither in it nor in the lacustrine deposits alluded to had any organic remains been found.

M. de Normand stated that Obsidian knives, like Mexican types, were found by him, with domestic implements cut out of volcanic stone, under 70 feet of tuff of the primitive volcano of Santorin; and he considered that before the formation of the first volcano ceramic pottery was brought to Santorin from foreign shores, and, of course, by sea.

Dr. Meryon remarked that the occurrence of the same type of implement in Europe and Asia proved a dispersion of the human race in very ancient times, and that man originated from one centre; while in later times a divergence of type in the worked objects was a result of the dispersion.

Mr. Prestwich was inclined to believe that greater physical changes had occurred in India since the Pliocene period than in Europe. The implements were so like those of Europe, that their fabricators seemed to have been taught in the same school.

Mr. Foote, in reply, stated that he regarded the laterite as a marine formation, because it occurred all round the coast. All the implements were quartzite, with perhaps one doubtful exception, which was formed of basalt. Stone circles and kistvaens had been found on the surface of the laterite in some localities.

2. " On worked Flint flakes from Carrickfergus and Larne." By G. V. Du Noyer, Esq.

These flakes have been found by the author in two very distinct positions, namely :—the older in the marine drift (sand and gravel) skirting the shores of the county Antrim and county Down, the maximum elevation being about 20 feet above the sea ; and the more recent in the subsoil clay at all elevations up to 600 feet, near Belfast, Carrickfergus, Larne Lough, and Island Magee. The former are of the rudest forms, highly oxidized or white on their entire surface, but, though imbedded in marine drift, having the chippings around the sides and angles generally sharp. The latter have a comparatively fresh look, though still possessing the characteristic porcellanous glaze ; they are regarded by Mr. Du Noyer as possibly the rough materials out of which the historic races in Ireland manufactured the spear- and arrow-heads which are found with their sepulchral and other remains.

3. " On the Diminution in the volume of the sea during past geological Epochs." By Andrew Murray, Esq., F.L.S.

In opposition to Sir Charles Lyell, the author submitted that, instead of the proportion of dry land to sea having always been the same, and its volume above the level of the sea a constant quantity, they are constantly increasing, while both the mean and extreme depths of the sea are constantly diminishing, the cause being the extreme affinity which water has for the constituent elements of minerals. In illustration of his view, he quoted the so-called upheaval of coral-islands as being really caused by a diminution in the volume of the sea.

4. " Has the Asiatic Elephant been found in a fossil state ? " By A. Leith Adams, M.B., F.G.S. With a Note by G. Busk, Esq., F.R.S., F.G.S.

An elephant's tooth in the possession of Dr. Fischer, of St. John, New Brunswick, which had been found in Japan at a distance of 40 miles from the sea-shore, between Kanagawa and Jeddo, and at the base of a surface coal-bed, appeared to the author referable to the Asiatic elephant ; and he accompanied his description of it by a drawing and plaster cast. In his note appended to the paper, Mr. Busk gave some further details of the characters exhibited by the cast, and agreed with Dr. Leith Adams in regarding it as probably referable to *Elephas Indicus* rather than *E. Armeniacus,* a fossil molar of which had been found in China ; but he concluded that it was the *antepenultimate* upper left molar, and not the *penultimate,* as inferred by Dr. Leith Adams.

5. " On the Characters of some new fossil Fish from the Lias of Lyme Regis." By Sir Philip de M. Grey Egerton, Bart., M.P., F.R.S., F.G.S.

The species described in this paper were the following :—

1. *Osteorachis macrocephalus,* gen. et spec. nov.—A Sauroid fish, chiefly remarkable for the massive dimensions and complete ossification of the bodies of the vertebræ, and characterized by the large size of the head and the multiplicity of the teeth.

2. *Isocolum granulatum,* gen. et spec. nov.—For elegance of form this fish can vie with the salmon of modern times, its contour being very similar. It bears the greatest resemblance to the Sauroid genus *Caturus,* but in the absence of the teeth it cannot be assigned with certainty to any particular family.

3. *Holophagus gulo,* spec. nov.—A cœlacanth fish, remarkable for its resemblance, especially in the contour of the head, to the Cretaceous genus *Macropoma,* and for substantiating Prof. Huxley's demonstration of the persistence of type presented by this family, which ranged from the Coal-measures to the Chalk.

4. *Eulepidotus sauroides,* gen. et spec. nov.—This first represents a genus uniting the Lepidoid and Sauroid families of Agassiz's Ganoid order—the teeth and the tail being Sauroid in character, while the fins are Lepidoid, and the scales partake of the characters of those structures in both families.

6. "Note on the Geology of Port Santa Cruz, Patagonia." By Capt. T. Baker, Lieut. Royal Naval Reserve.

This note accompanied some specimens of fossil shells obtained by the author from the cliffs of the western arm of the river Santa Cruz, the stratification of which he described. The shells are for the most part referable to the Tertiary species from Patagonia previously obtained by Mr. Darwin.

7. "On the Jurassic deposits in the N.W. Himalaya." By Dr. F. Stoliczka, F.G.S., of the Geological Survey of India.

The author described the following strata as composing the Jurassic rocks in the north-west Himalayas:—

1. Lias.... { *a.* Lower Tagling limestone.
          { *b.* Upper Tagling limestone.
2. Dogger { *c.* Jurassic slates.
          { *d.* Spiti shales.
3. Malm? *e.* Gieumal sandstone.

The object of the paper was to show, in opposition to Mr. Tate's assertion to the contrary, that the Indian Jurassic formation could clearly be subdivided, and that in some measure the subdivisions correspond with those of the European Jura.

8. "On a true Coal-plant (*Lepidodendron*) from Sinai." By J. W. Salter, Esq., A.L.S., F.G.S.

The fossil described was received by Sir R. I. Murchison some years ago. The author regarded it as an infallible indication of the pesence of the true northern Coal-formation, with species like those from the Erekli coal. The proposed name of the species is *Lepidodendron mosaicum.*

9. "On some Fossils from the Menevian Group." By J. W. Salter, Esq., A.L.S., F.G.S., and H. Hicks, Esq.

The authors, after describing the localities and stratigraphical relations of the Menevian group, proceeded to describe the following species:—

Paradoxides aurora, Salter, represented by a few imperfect heads, unattached pleuræ, &c. Localities, Porth-y-rhaw and St. Davids.

P. Hicksii, Salter. This species presents a singularly inter-

mediate character, reminding us equally of *Paradoxides* and *Ano-polenus*.

Conocoryphe bufo, Hicks, represented by a few separate heads and one with six body-rings attached. Localities, Porth-y-rhaw and St. Davids.

C. applanata, Salter. Young specimens show all the metamorphoses observed by Barrande. The characters of such genera as *Agnostus* and *Microdiscus* are as clearly seen in the embryo of *Conocoryphe* as in the adult state of those genera. Localities, Porth-y-rhaw, St. Davids, Maentwrog, and Dolgelly.

C. (?) *numerosa*, Salter. Of this species, a part of the head and six thoracic rings have been found. These, however, show characters sufficient to indicate that it is specifically, if not generically, distinct from the others. Localities, Porth-y-rhaw and St. Davids.

10. " On Earthquakes in Northern Formosa." By H. F. Holt, Esq., H.M. Consul at Tamsuy.

The first shock felt in the northern end of the island took place on the morning of the 18th of December, 1867. Many buildings were destroyed and many lives lost in Tamsuy. About fourteen minor shocks were felt during the same day, and on the 20th another violent shock occurred.

At Kelung the whole harbour was left dry, and the water returning in one vast wave rushed into the town itself. Large landslips have taken place, and several villages between Kelung and Tamsuy have been destroyed.

11. " Memorandum on the Coal-mines of Iwanai, Island of Yesso, Japan." By A. B. Mitford, Esq.

The mines lie about two miles inland from the village Kaianoma. Four seams of coal have been discovered, which are from one to six feet thick. The coal is soft, yields from ten to twelve per cent. of ash, and from thirty to thirty-five per cent. of gas. It sends out thick black smoke when first lighted, but afterwards burns with a clear strong flame, and leaves no clinker.

12. " On a new species of Fossil Deer from Clacton." By W. Boyd Dawkins, Esq., M.A., F.R.S., F.G.S., &c.

This species (named *Cervus Brownii* by the author) is unlike any other species excepting *C. dama*, to which it is closely allied. The antlers, however, have the third tyne present on the anterior portion, while in the Fallow deer it is entirely absent. From the presence of *Rhinoceros Merkii* and *Elephas antiquus* in the Clacton deposit, and from the absence of Arctic species, the author regarded it as forming a term in the series of strata to which the Lower Brickearths of the Thames valley belong, and as deposited before the immigration of Arctic animals into Great Britain.

13. " On a new species of Fossil Deer from the Norwich Crag." By W. Boyd Dawkins, M.A., F.R.S., F.G.S., &c.

Cervus Falconeri, Dawkins, spec. nov. The brow tyne differs from that of *C. dama* and of *C. Brownii* in being removed from the base, and situated in a different plane from the second and third

tynes; in this it is allied to *C. tetraceros.* The straightness of the beam separates it from the species to which he had compared it; and it is further separated from *C. tetraceros* by the absence of deep wrinkles. The small amount of palmation in *C. Falconeri* is greatly increased in *C. Brownii,* and reaches its maximum in *C. dama.*

14. " Notes to accompany a section of the Strata from the Chalk to the Bembridge Limestone at Whitecliff Bay, Isle of Wight." By T. Codrington, Esq., F.G.S.

In these notes the author described in detail the beds which are comprised in the section exhibited in Whitecliff Bay, and which he had carefully measured at low water. Comparing it with the Alum-Bay section measured by the officers of the Geological Survey, he found the total thickness of the beds from the chalk to the base of the fluviomarine series to be the same in both, although the thicknesses of the component formations differ considerably.

15. " On the Graptolites of the Coniston Flags, with notes on the British species of the genus *Graptolites.*" By H. A. Nicholson, D.Sc., M.B., F.G.S., &c.

The author, after remarking upon the prevalent differences of opinion regarding the stratigraphical position of the Coniston Flags, proceeded to describe the following species :—

Diplograpsus palmeus, *Barr.*	Graptolites Sedgwickii, *Portl.*
D. folium, *His.*	G. fimbriatus, *Nich.*
D. angustifolius, *Hall.*	G. Nilssoni, *Barr.*
D. confertus, *Nich.*	G. tenuis, *Portl.*
D. tamariscus, *Nich.*	G. discretus, *Nich.*
D. pristis, *His.*	G. Bohemicus, *Barr.*
Retiolites perlatus, *Nich.,* nov. sp.	G. priodon, *Bronn.*
Rastrites Linnæi, *Barr.*	G. colonus, *Barr.*
Climacograpsus (Diplograpsus)	G. sagittarius, *Linn.*
teretiusculus, *His.*	G. turriculatus.
Rastrites peregrinus, *Barr.*	G. Sedgwickii, *Portl.*
Graptolites lobiferus, *M'Coy.*	——, var. spinigenis, *Nich.*

16. " On the ' Waterstone Beds' of the Keuper, and on Pseudo-morphous Crystals of Chloride of Sodium." By G. W. Ormerod, Esq., M.A., F.G.S.

Between Salcombe Mouth and the River Sid, and between Bud-leigh-Salterton and Littleham Bay, several beds of ripple-marked " Waterstone " occur, and also pseudomorphous crystals of chloride of sodium. A small fragment of Waterstone exhibited apparently traces of reptilian remains. In conclusion, the author drew attention to the fact that pseudomorphs occur over the greater part of the Triassic area in England.

17. " On the discovery of the remains of Pteraspidian Fishes in Devonshire and Cornwall, and on the identity of *Steganodictyum Cornubicum,* M'Coy, with *Scaphaspis* (*Archæoteuthis*) *Dunensis,* Rœmer." By E. Ray Lankester, Esq.

A specimen labelled " *Pteraspis,*" from the Lower Devonian slates of Mudstone Bay, in the collection of the late Mr. Wyatt-Edgell,

was at once referred by Mr. Salter to the *Steganodictyum* of M‘Coy; and on further research he concluded that M‘Coy's supposed sponge is actually the cephalic plate of a Pteraspidian fish. The author fully endorsed Mr. Salter's determination, and inferred that the specimens of *Steganodictyum Carteri* are really head-plates of true *Cephalaspis*.

18. " On the Geological peculiarities of that part of Central Germany known as the Saxon Switzerland." By the late Capt. James Clark.

The author described in detail the rocks of which the district under consideration is composed, namely :— 1, the Upper Quader Sandstone ; 2, Pläner Limestone ; 3, Pläner Marl ; 4, Lower Quader Sandstone ; and gave a list of their chief fossils.

The peculiarities of this region consist, first, in the abrupt and marked variations of altitude without any corresponding inclination or dislocation of the strata ; secondly, in the remarkable regularity of the fissures by which the rocks are divided, which cross them at right angles ; thirdly, in the phenomena observable along the line of separation between the Quader and the Lusatian granite, the Quader being overlain by the granite and syenite ; fourthly, in the disposition of the basalt, which rises through the granite and the stratified rocks above, indurating the latter, but not contorting them.

XXI. *Intelligence and Miscellaneous Articles.*

ON THE TEMPERATURE OF FLAMES AND DISSOCIATION.
BY E. VICAIRE.

DURING the combustion of a gaseous mixture in an enclosure which is impermeable to heat, the heat evolved is employed solely in heating the mass, and we may easily calculate the temperature which it will have attained after the combustion of a certain fraction of the combustible element.

Supposing this combustion to be complete, we obtain the ordinary formulæ of the temperatures of combustion. But these formulæ furnish results far higher than the temperatures which are actually observed, and the beautiful investigations of M. H. Sainte-Claire Deville clearly show us the reason of this ; it is, that after a certain point the elevation of the temperature places an obstacle in the way of a more complete combustion, because no combination can take place without causing a dissociation which exactly compensates it.

If we have determined the temperature by experiment, we easily deduce from it the quantity of gas that has been burnt, by the same equation which, if we knew the quantity burnt, would give the temperature.

But this equation, which I establish in a rather more general manner than has hitherto been done, does not take the place of the old formulæ of combustion : it does not allow us to foresee the tem-

perature of combustion of a given mixture ; for it includes two magnitudes which are equally unknown beforehand—the quantity burnt, and the temperature.

To determine these two magnitudes for a given mixture is the problem which I have set myself, assuming the phenomenon of dissociation to be known in all its details. Although this assumption is far from having been realized, the solution of this problem possesses henceforth a certain amount of interest, inasmuch as it enables us to account for the circumstances which may influence the temperature of combustion, and the direction of this influence. It will be seen, moreover, that it suggests various means of studying dissociation experimentally.

Let us take, in the first place, a mixture of equal equivalents of oxygen and hydrogen ; let us call c the specific heat of the mixture, c' that of aqueous vapour, and let k, at a given moment, be the fraction of the mixture which is not yet burnt ; we then easily establish the proportion

$$[kc+(1-k)c']t=(1-k)3240,$$

whence

$$k=\frac{3240-c't}{3240+(c-c')t}. \qquad \cdots \cdots \quad (1)$$

Taking t as the abscissa and k as the ordinate, this equation is that of an hyperbola each point of which, in the part with positive coordinates, represents one of the states through which the mixture would successively pass if the combustion could become complete. The ordinate defines the composition of the mixture, the abscissa gives the temperature.

On the other hand, let us consider aqueous vapour brought to a gradually increasing temperature. Let u be the tension of dissociation at a given moment—that is to say, the fraction which has been transformed into a mixture of chemical equivalents of oxygen and hydrogen ; if we suppose the pressure constant, u will be a function of the temperature alone, or

$$u=f(t). \qquad \cdots \cdots \cdots \quad (2)$$

This equation will be that of a curve which will also represent the successive states of the aqueous vapour.

From the position of the points where the curves (1) and (2) intersect the axis of the t's and the horizontal $k=1$, they must of necessity intersect each other between these two lines.

The point of intersection corresponds to a moment at which the gaseous mass in combustion is identical in composition and temperature with dissociated water. Now this is in a state of equilibrium which it is incapable of modifying of itself ; this is therefore the case with the gaseous mass ; that is to say, always supposing it in an enclosure impermeable to heat, it must remain indefinitely in the same state. Here, therefore, we have the stationary condition, and the corresponding temperature is the *actual temperature of combustion*.

Thus this temperature will be given by the equation

$$\frac{3240-c't}{3240+(c-c')t}=f(t). \quad . \quad . \quad . \quad . \quad . \quad (3)$$

If the gaseous mixture, instead of being originally at zero, contained more than at zero, a quantity of heat, v, positive or negative, the equations would become

$$k=\frac{3240+v-c't}{3240+(c-c')t}=f(t). \quad . \quad . \quad . \quad . \quad (4)$$

If, instead of being dry, the mixture, before any combustion, contained a fraction q of its weight in the state of water, we should have

$$k=\frac{3240(1-q)+v-c't}{3240+(c-c')t}=f(t). \quad . \quad . \quad (5)$$

In these various cases nothing is ever present but aqueous vapour and explosive gas; if we suppose the former to be always the same, the dissociation will only change from one case to the other by the effect of temperature. The function $f(t)$ is therefore always the same. If, therefore, we place ourselves successively in different cases, varying v and q (that is to say, the initial temperature and humidity), and observe the temperature of combustion, we shall deduce therefrom different values of $f(t)$; that is to say, we shall get the law which connects the tension of dissociation with the temperature under the pressure in question.

The equations (4) are equally true at any moment that the quantity of heat v is added or deducted. They agree therefore, supposing v to be negative, with the successive states of the mass when it cools from the maximum of temperature. We are thus led to another method of obtaining values of $f(t)$,—namely, observing simultaneously, during the cooling of the mass, the quantities of heat which it abandons and the temperatures through which it passes.

If each kilogramme of more or less hot and moist explosive gas were mixed with a weight p of a gas not capable of entering into reaction, and of specific heat c'', we should have

$$k=\frac{3240(1-q)+v-(c'+pc'')t}{3240+(c-c')t}=f_1(t).$$

I here introduce this new function $f_1(t)$, because we may assume, until the contrary is proved, that the presence of a foreign gas modifies the dissociation.

It is evident that, by varying only v and q, we may determine the function $f_1(t)$. By subsequently varying the nature and the quantity of the intermixed gas, we shall see how these conditions modify the dissociation.

Assuming the identity of $f(t)$ and $f_1(t)$, we easily see that if we compare two mixtures for which the temperature of *total* combustion (that given by the ordinary formulæ) is the same, but one dry and containing a foreign gas such as nitrogen, the other with no

foreign gas, but humid, the *real* temperature of combustion will be less for the latter.

It is clear that these formulæ apply to oxide of carbon with mere numerical modifications. They likewise suit, whatever be the pressure, provided this is constant in each case.

Without trying prematurely to indicate the arrangements by which I hope to realize some, at least, of the cases under consideration, I pass to the examination of a recent memoir by M. Bunsen on the present question.

M. Bunsen detonates gaseous mixtures in a valved eudiometer, and calculates the temperature from the pressure which is developed at the moment of the explosion. This case differs in two points from those which we have considered : in the first place, we must introduce into the formulæ specific heats at constant volumes, and not at constant pressures ; and then the pressure does not remain constant, but it increases up to the moment of the maximum of temperature. These two circumstances tend to augment the real temperature of combustion, at least if we assume that pressure tends to diminish the tension of dissociation.

It will thus be understood how M. Bunsen finds for the gas of the pile 2800°, and M. Deville, operating under the ordinary pressure, only 2500°.

I next discuss M. Bunsen's theoretical conclusion, according to which dissociation is not a continuous phenomenon, but one varying suddenly at certain temperatures, in the interval of which it remains constant, varying moreover in such a manner that there is always a simple proportion between the dissociated part and that which is not dissociated. I show that this conclusion has but little probability *à priori* ; for this instantaneous production of a phenomenon which induces an evolution or an absorption of heat, when the exchange of heat with surrounding bodies is necessarily continuous, must cause sudden variations of temperature such as we do not observe.

M. Bunsen shuns the difficulty by assuming that, during the passage from one simple proportion to another, the temperature remains constant. But we ought to observe this period of constancy. Thus in the flame of a blowpipe with mixed gases, of which the various parts offer precisely the successive conditions of a mixture in combustion which is cooling, we ought to observe a very appreciable space during which the temperature would be constant. Now the accurate experiments of M. Deville upon the blowpipe with oxide of carbon indicate a continuous decrease of both the temperature and the dissociation.

Moreover, during this period of invariable temperature, which should represent a very noticeable fraction of the total duration of the refrigeration, there would be a gradual, and not a sudden, passage from one simple proportion to another. The simple proportion would therefore not be an absolute natural law.

Finally, even the discussion of the experiments leaves, at least, much doubt as to the legitimacy of the conclusion.—*Comptes Rendus,* December 28, 1868, vol. lxvii. pp. 1348–1352.

ON A FRICTION AND INDUCTION ELECTRICAL MACHINE.
BY F. CARRÉ.

The author exhibited to the Academy of Sciences a new generator of electricity founded upon statical induction reduced to its simplest expression, the action of which he considers may aid in more clearly defining this phenomenon.

It consists of the friction-plate of the old electrical machine turning slowly between two cushions; above and parallel to this plate turns a larger disk of non-conductive matter, in such a position that the superior and inferior sectors of the two plates reciprocally cover from two-fifths to three-fourths of each other's radii.

The inferior plate fulfils the function of an inductor, the charge of which is kept constant by its continuous passage between the two cushions; it acquires positive electricity. In front of the inferior sector of the induced disk is a vertical comb connected with a conductor which becomes charged with positive electricity; a second comb placed diametrically collects the negative electricity which is poured out upon the disk by the lower comb.

From the direct action of the inductive plate at its maximum of charge, the apparatus is but slightly sensitive to the humidity of the atmosphere, and the induced disk furnishes an abundant evolution of electricity of considerable tension. Sparks of 15 to 18 centimetres may be obtained with a machine having plates 38 and 49 centimetres in diameter; and the interposition of a condenser increases their length.—*Comptes Rendus*, December 28, 1868, p. 1341.

ON THE SHAPE OF THE FLAME OF A BUNSEN'S BURNER.
BY A. POPPE.

On closely viewing the flame of an ordinary Bunsen's burner, we are at once struck by the peculiar flickering or pulsation of the flame, to which any definite rhythm seems wanting. I investigated the cause of this phenomenon, and the true form of the flame, by the method which Magnus used in his investigation on the constitution of the jet of water. Between the eye and the flame is interposed a circular disk, in which is cut a narrow slit in a radial direction, so that the whole length of the flame is seen when the slit is parallel to it. On rotating the disk, when the velocity has acquired a certain rapidity it will be observed that that flickering is due to an undulatory motion, which consists in a very rapid and regular succession of enlargements (loops) and contractions (nodes) of the flame. If the disk makes one rotation in the time required for the succeeding wave to take the place of the preceding one, the flame appears immoveable, and the true form of the flame, which is *that of an unduloid with a circular section*, is at once plain. From the measurable distance of the two loops, and the given velocity of rotation of the disk, the actual velocity of propagation of the wave-motion may be calculated.—Poggendorff's *Annalen*, No. 10, 1868.

XXII. *Historical Notes on some Phenomena connected with the Boiling of Liquids.* By CHARLES TOMLINSON, *F.R.S.*[*]

1. WHEN water is heated to that temperature at which its tension equals the whole pressure of both air and vapour on its surface, and it begins to emit steam not only from its surface (as it did before), but from all parts of its depth, it is said to be boiling. The boiling-point of any liquid means, therefore, the temperature at which its evaporating tendency equals the pressure of the atmosphere at the time—or the lowest temperature at which its vapour can have the elasticity of common air.

2. During many years after the invention of the barometer, and the consequent discovery of atmospheric pressure, it was supposed that the above statements contained a sufficient account of boiling. It was not until the irregular shifting of the boiling-point in thermometers under a constant pressure had been insisted on by such men as De Luc, Shuckburg, Horsley, and Cavendish, that steps were taken to determine the conditions on which that instrument should be constructed. The Royal

[*] Communicated by the Author.
[In preparing the paper " On Boiling Liquids " recently read before the Royal Society, I was led to consult a good many books and memoirs, and to accumulate a number of historical details which, though not adapted to the ' Proceedings ' of a Society whose chief object is to assist the progress of living science, may yet, if briefly stated, be acceptable in the Philosophical Magazine, which performs the useful functions both of critic and of observer. The insertion of these historical details may serve to correct some errors that have crept into our text-books.]

Society gave the sanction of its high authority to a Report on the subject prepared by some of its most distinguished members, and published in the Transactions for the year 1777.   In this Report the want of fixity in the boiling-point of water under a constant pressure is noticed, and the cause is referred to the depth of water in the vessel, which causes it to boil in gusts.   It was found, however, that the elasticity of the steam from boiling water fairly represents the atmospheric pressure, and it was recommended that the water be boiled in a metal vessel constructed so as to allow the bulb, and nearly the whole of that part of the stem that contained mercury, to be surrounded by the steam.

3. I am not aware that in this Report, or in the results previously published that led to it, it is anywhere stated that the nature of the vessel influences the boiling-point.   That fact was distinctly brought forward by M. Achard in 1785*.   Distilled water was boiled in a brass cylindrical vessel by means of a spirit-lamp, the thermometer-bulb being within half an inch of the bottom.   Under these conditions the temperature was constantly varying, although the water appeared to boil equably.   Blowing on the side of the vessel, opening and shutting a door, or anything that produced an agitation in the air caused a fall in the thermometer of $1\cdot12^\circ$ R.   When the water was boiled in a matrass of white glass, there was no variation in the boiling-point as indicated by the thermometer.   Achard performed a large number of experiments on the boiling-point of water in vessels of silver, brass, porcelain, earthenware, glass, &c., many of them being articles in domestic use; and he gives drawings of them in three folding plates, with measurements of their dimensions, to justify his conclusion (afterwards found to be erroneous) that in vessels of the same material the temperature of boiling water varies according to the size of the opening.   There is no doubt, however, as to the influence of the vessel on the boiling-point. He says:—"Le degré de chaleur de l'eau en ébullition dans différens vases est différent pour la même pression atmosphérique quoique les vases soient chauffés de la même manière et qu'ils se trouvent dans le même bain de sable."   His idea was, that, metal being a good conductor, the heat readily escaped from it when the air was agitated, while glass retained its heat, and hence the oscillation of the mercury in the one case and its fixity in the other. He concludes his memoir with the remark "that the experiments prove that the degree of heat of boiling water under an equal

pressure of the atmosphere is not a fixed term, but that many circumstances cause it to vary; that it is much more inconstant in vessels of metal than in vessels of glass; and that the action, more or less direct, of the external air on the sides of the vessels, especially when of metal, as upon the surface of the water, produce considerable changes in the degree of heat that it may receive in boiling. As the construction of the thermometer depends on the fixity of the boiling-point, it is not surprising that thermometers made on this supposition, with the greatest possible care, should not always agree."

4. I have given these details at some length, because French and English writers of authority assign to Gay-Lussac the merit of Achard's discoveries. Biot seems to have led the way in this respect. Writing in 1816*, he says, in reference to the boiling of water, " Il y a aussi quelques différences dans le degré de l'ébullition selon la nature des vases que l'on emploie, et selon celle des substances qui se trouvent mêlées à l'eau, même quand elle ne peut les dissoudre. Cette remarque est due à M. Gay-Lussac."

5. The effect of insoluble substances on the boiling-point was also first noticed by Achard, in 1784†. The water was boiled in a glass vessel; and when the mercury in the thermometer was steady, a drachm of the solid to be tried was thrown in, and the effect noted in tenths of a degree on Réaumur's scale. When the temperature had again become steady, a second drachm of the substance was thrown in, and so on until no further effect was apparent. A large number of substances were tried in this way, and the results are given in Tables occupying fourteen quarto pages. Each Table contains six columns, for recording the name of the substance, the height of the barometer, the boiling-point of the water before the addition, the weight of the water, the weight of the substance added, and, lastly, the effect on the thermometer in tenths of a degree Réaumur. Thus 1 drachm of iron-filings lowered the thermometer ten tenths, or −10 as Achard writes it; a second drachm had no further effect; copper-filings −8, tin-filings −13, white sand 0 to −3, calcspar −13, quicklime −9, rosewood −11, limestone in powder −13, the same in a lump −3, bismuth in powder −12, the same in fragments −8, and so on. Achard does not pretend to offer any satisfactory explanation of these results, but he distinctly claims the merit of having originated them.

6. The effect of soluble substances on the boiling-point was

* *Traité de Physique*, vol. i. p. 42.
† Berlin Memoirs, 1784, published 1786. The following is the title of the memoir:—" Sur l'effet produit par l'addition de différens corps à l'eau, relativement au degré de chaleur dont elle est susceptible dans l'ébullition."

clearly made out during the fine experiments undertaken by Dalton, Watt, Robison, Southern, and others for determining the pressures of saturated steam at different temperatures above and below the standard boiling-point. It was noticed that if a minute portion of soda, or of some salt soluble in water and not capable of rising in vapour with it, be allowed to ascend to the top of the mercury, the column rises, thereby indicating a diminished pressure of steam, although the soda has not touched it, but remains covered by the layer of water on the top of the mercury. This shows that the elasticity depends not merely on the temperature and the nature of the vapour, which are both unchanged, but on the nature of the liquid. The adhesion of the soda to the water tends to restrain the water from evaporating, and this tendency is a measurable force and here measured ; for it partly balances the tension of the water, or its tendency to emit steam, and thus makes the steam-emitting tension of a solution of soda measurably less than that of pure water at the same temperature. As the difference remains at all temperatures, the solution must always be made hotter than pure water in order to give steam of the same elasticity.

7. The effect of air dissolved in the water on the boiling-point was noticed in minute detail by De Luc* in 1803, not indeed for the first time ; for in his previous works, published in 1772 and 1786†, he had described the principal experiment on which his remarkable theory was based. He says :—" Le phénomène de l'ébullition est produit par des bulles d'air que la chaleur dégage du liquide . . . . ; quand on a préalablement purgé l'eau de tout l'air qu'elle contenoit, elle ne peut plus bouillir; et la raison en est que les vapeurs ne peuvent se former qu'à des surfaces libres. Les bulles d'air qui se rassemblent dans son sein y produisent des solutions de continuité ; c'est-à-dire, ces surfaces libres nécessaires ; mais quand l'eau est purgée d'air les vapeurs ne peuvent se former qu'à sa surface extérieure " (*Introduction*, &c. vol. i. page 247). De Luc had already described, in 1772, with great minuteness of detail an experiment in which a matrass containing water, and also a small thermometer, had the upper part of its tube drawn out into a capillary bore, and, the matrass being heated in hot water, the air as it accumulated in the fine tube was got rid of by an ejection of steam, and the tube was sealed. The tube was also subjected to percussion during a long time, and the process of heating was continued

* *Introduction à la Physique terrestre par les Fluides expansibles.* Paris, 1803.

† *Recherches sur les Modifications de l'Atmosphère.* Geneva, 1772. See chapter 10 of the Supplement to vol. ii. *Recherches sur les Variations de la Chaleur de l'Eau bouillante. Idées sur la Météorologie.* London, 1786.

until more air got into the fine tube, when it was opened and the
air expelled. After continuing these operations of shaking and
heating the tube in order to get rid of the air, the tube was
raised to 212°; and the point being broken so as to reestablish
atmospheric pressure, the water was further heated to $234\frac{1}{2}$° F.
without boiling*.

8. Proceeding in the order of time, we come to Gay-Lussac†.
While engaged in his experiments on solubility, he seems to
have rediscovered the two facts respecting the influence of the
vessel on the boiling-point, and also of the effect of an insoluble
body in lowering the boiling-point. But it is remarkable that
in both his papers Gay-Lussac refers to Achard's results, or
rather to his faulty conclusions, with a view to contradict them,
without giving him credit for those points that were true. It is
quite possible that Achard's fame was more injured by his friends
than his opponents, since an attempt had been made by Gmelin
and others, in answer to Gay-Lussac's paper of 1812, to show
that in a number of vessels of different material, all sunk to the
same depth in a sand-bath, water boiled at the same temperature‡;
while in 1817 Muncke stated§ that copper-filings appeared to
have no influence in lowering the boiling-point, and that sand did
so only to the extent of one-tenth of a degree. Gay-Lussac in 1817
noticed these papers, and reasserted the original facts with even
more decision than he had done in 1812. But even in this his
first notice of the subject, his language is not to be mistaken.
His paper is on Deliquescence, and he says:—"In determining
the boiling-point of saline and acid liquids I observed a very
singular phenomenon which deserves to be known. Water or
any other liquid boils later in a glass vessel than in a metallic
one, except when we put into the former some turnings of iron,
copper, or other metal, or carbon in powder, or pounded glass.
The difference in the case of water may amount to 1·3° C. and
upwards." In 1817 he states‖ that water boils later in glass
and earthenware (*faïence*) than in metal vessels. He does not
give the measure of the difference, but believes it to vary with the
nature of each body, and, with the same substance, according to
the nature of its surface: "car il est probable qu'elle dépend à

* De Luc also dropped water into oil heated to 82° or 90°, and even 100° R.;
but he was by no means satisfied that the water ever attained this tempe-
rature. He says:—"Ces gouttes d'eau, renfermées dans l'huile, pouvoient
être dans un état particulier" (see paragraph 993 of the *Recherches*). That
is, they were probably in what would now be called the "spheroidal state,"
which De Luc understood and accurately describes in § 1007.
  † *Annales de Chimie*, vol. lxxxii. p. 171.
  ‡ Schweigger's *Journal*, vol. xxvii. p. 27.
  § Gilbert's *Annalen*, vol. lvii. p. 215.
  ‖ *Ann. de Chim. et de Phys.* vol. vii. p. 307.

la fois et de la propriété conductive pour le calorique et du poli des surfaces." When a flask half full of water is in a state of ebullition, a peculiar noise shows that the boiling goes on with difficulty; large bubbles of steam proceed from certain points only of the surface, and a thermometer plunged into the water shows frequent variations. In a tinned-iron vessel, on the contrary, the bubbles of steam are not so large, but they are more numerous, the variations of the thermometer are less considerable, and the boiling point is not so high. If, however, water be boiled in a glass vessel and a few pinches of iron-filings be put into the vessel, the boiling instantly goes on as in a metal vessel. In the absence of this aid, the quantity of steam formed is proportional to the excess of the temperature of the water over that of its boiling-point. The temperature falls to $100°$ C. with a sort of explosion, the steam having to overcome the cohesion of the liquid and its resistance to change of state. This cohesion, or viscosity of the liquid, must exert a great influence on its boiling-point, since the steam has to overcome not only the atmospheric pressure, but also the cohesion of the liquid molecules. Then, again, the adhesion of the liquid to the vessel must be a force analogous to its viscosity. The conducting-power of the vessel for heat and the nature of its surface must also exert considerable influence on the boiling-point of water. Water boils more easily in a glass vessel into which powdered glass has been thrown, than in a glass containing nothing but water.

9. Gay-Lussac illustrates the greater or less ease with which steam escapes from boiling water by referring to the readiness with which carbonic acid escapes from fermented liquors, such as beer or champagne, especially when a bit of paper or a crust of bread, &c., is introduced, or simply by stirring. Carbonic acid is mostly disengaged from the sides of the vessel, and especially from parts containing asperities. The bubbles increase in size in traversing the liquid, because they establish in it a solution of continuity which is very favourable to the disengagement of the gas. It is easy to explain by these facts the phenomena of *soubresauts* or "jumping ebullition," or "kicking." When the liquid is above the boiling-point, it is in a forced state; and in an instant a burst of steam is formed, the liquid is thrown out, and the vessel itself raised. This is especially the case in distilling sulphuric acid; but if some pieces of platinum wire be put into the retort, the distillation then becomes easy.

10. It is impossible to read this paper without being struck with the large amount of suggestion contained in it. Indeed Gay-Lussac's imagination was so lively that it seldom left him satisfied with one explanation when he had to discuss some new

point, and the varied suggestions thrown out by him have each served as a basis for further inquiry by subsequent observers. But the difference between him and some of them consists in this, that he would originate an idea, examine, and discard it when seen to be no longer tenable; they clung to the idea even though it carried them into the region of error. It is the distinction between genius and talent, that the one originates capital, and the other trades with it.

11. In 1825 Dr. Bostock published some facts respecting the boiling-point of ether*. Ether (spec. grav. 0·755) in a matrass over a spirit-lamp boiled at 112° F.; but in a test-tube put into hot water it did not begin to boil until it had reached 150°, and on one occasion 175°. Some bits of cedar-wood were put into the ether, and it boiled at 110°; the wood was covered with bubbles, until it got soaked and sank. Bits of quill, feather, wire, pounded glass, &c. also lowered the boiling-point considerably. After stating that copper-filings, chips of wood, &c. appeared to produce ebullition in the ether after it had ceased to boil, Dr. Bostock says, "Plunging a thermometer into ether caused the production of the bubbles at a temperature many degrees below the point at which ebullition took place without the thermometer; but the effect of the thermometer after a short time was no longer perceptible, and I observed that by alternately plunging the thermometer into the ether and removing it from the fluid, the bubbles were produced at each immersion." The various nuclei above referred to are said to act by carrying down air into the ether, and as soon as the air is discharged they cease to act.

12. Dr. Bostock's interesting paper called attention to some processes in the useful arts adopted with a view to facilitate the process of boiling. Thus Mr. Bald† notices a practice among the engine-keepers of Scotland, when the ordinary supply of steam cannot be raised from the boiler, of throwing in about a bushel of the radicles of barley, separated during the process of cleaning the malt and called " comings." When steam is again raised, the effect is immediately apparent ; " for not only is there a plentiful supply of steam for producing the full working speed of the engine, but an excess going to waste at the safety-valve. This singular effect will continue several days." So also in distilling ardent spirits on a large scale, when converting the fermented wash into "low wines," it is usual to throw a bit of soap into the still every time it is charged. This causes the steam to rise more quickly, and to be freer from the residual matter of the process.

* Annals of Philosophy, N. S. vol. ix. p. 196.
† Edinb. Phil. Journ. vol. ii. p. 340.

13. Attention was also called to a conclusion arrived at by Œrsted*, that gas resulting from chemical decomposition is never disengaged from a liquid except in contact with some solid body ; and he proposes to apply this principle to the disengagement of vapours. He says :—" If a metallic wire be suspended in a boiling fluid, it instantly becomes covered with bubbles of vapour." To show the application of this fact, ten pounds of brass wire, one-fifth of a line in diameter, were put into a still containing ten pints of brandy ; seven pints came over at a heat which without the wire would have sent over only four.

14. In 1835 Le Grand† remarked that " when water is boiled in a glass vessel the boiling goes on at first regularly, with the production of numerous bubbles, and without noise ; but as soon as the water has lost the greater part of the air that it held in solution matters become changed; the ebullition takes place in intermittent bursts, accompanied with noise, and the thermometer experiences considerable oscillations. This is known as *bumping*. Many salts in small quantity prevent it in a remarkable manner. Others favour it highly, especially the neutral tartrate of potash. Platinum is generally used to prevent it, on account of its unalterable character ; but this is a mistake ; for the moment a small quantity of platinum-filings is added, ebullition is facilitated, on account of the air introduced ; but as soon as this air is disengaged the bumpings are resumed."

15. In 1837 some curious experiments were recorded by Schönbein ‡, in which the effect of gas generated in the midst of water near the boiling-point was shown. For this purpose 1 per cent. of sulphuric acid was added to the water, and when it was boiling the lamp was removed and some bits of iron or zinc wire were thrown into the flask. Ebullition set in again immediately, and continued during some seconds. If the wire was first rolled up into a coil before being thrown in, the effect was so considerable that a portion of the contents of the flask was thrown out. Metals that do not disengage hydrogen, and from whose surface the adhering film of air has been removed by dipping them into boiling water (such as platinum, gold, silver, &c.), will not disengage any vapour from water near the boiling-point.

16. Schönbein's explanation of the effect of zinc in the acid water did not depend, as we should have supposed, on the effect of air or gas in diminishing the cohesive force of the liquid particles, but his theory rested entirely on the principle of heat overcoming pressure. He reasons in this way :—Hydrogen on being liberated has the same pressure as the atmosphere ; the

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* Gehlen's *Journal für Chemie,* vol. i. pp. 277–289.
† *Ann. de Chim. et de Phys.* vol. lix. p. 426.
‡ Pogg. *Ann.* vol. xl. p. 391.

elasticity of the vapour of water that has just left off boiling we may suppose to be one-hundredth less than the pressure of the air ; hence 1 volume of hydrogen and 99 volumes of vapour would produce a mixture equal in elasticity to the atmospheric pressure *.

17. Some of the German text-books† on physics assign to Rudberg the honour of an important observation made in 1837, namely, that although water boils at a higher temperature in a glass than in a metal vessel, yet the temperature of the steam is in both cases the same, the pressure being the same. This fact was known to Cavendish and the other eminent men who presented the Report on Thermometers to the Royal Society already referred to (2).

18. In 1842 Marcet published a long memoir " On certain circumstances that influence the Boiling-point of Water"‡. The conditions of the case are thus stated :—Boiling takes place in any liquid at the moment when the repulsive force of heat is sufficient to overcome the effect of cohesion among the particles of the liquid *plus* the atmospheric pressure. But if the liquid is in a vessel of which the adhesion of the sides to the particles of the liquid is more than the cohesion of the latter among themselves, this adhesion must be overcome, and more heat will be required than if the simple cohesion of the particles were concerned.

19. Marcet considers that iron, zinc, and other substances tend to lower the temperature of boiling water, because they have a less molecular adhesion for water than glass has. If the vessel be coated with a thin layer of sulphur, gum-lac, or any similar substance that has no sensible adhesion for water, the boiling-point is lowered, and the temperature of the water and of the steam are identical. In such case the boiling-point may be lower by some tenths of a degree than in metal vessels. The boiling-point varies in flasks made of the same glass, and even in the same flask at different times, irrespective of pressure. Differences were also noticed between flasks fresh from the maker and those that had been used for experiments. In such as had held

* Schönbein repeats Bostock's experiments, of course without being aware of it. He says, " bits of wood are remarkably active so long as their pores are full of air, but not at all when this is expelled." And again :—" All bodies that contain air or liberate it, set air or gas free from their solutions." This is precisely the theory of M. Gernez, " On the Disengagement of Gases from their Supersaturated Solutions,"as given in the *Comptes Rendus* for November 19, 1866, pp. 883–887, and examined by me in the Philosophical Magazine for August 1867.

† See Eisenlohr, *Physik*, 1860, p. 356. Rudberg's paper is on the Construction of Thermometers. It is inserted in Poggendorff's *Annalen*, vol. xl. p. 49, and is more modest in its claims for originality than the books referred to.

‡ *Annales de Chimie et de Physique*, ser. 3. vol. v. p. 449.

sulphuric acid the boiling-point of water rose to 106° C.; so much had the internal surface of the glass become modified by the acid; but by simply heating the flask to 300° or 400° C. a similar result was obtained.   The conclusion arrived at was that " the effect was due to a species of molecular modification of the surface of the glass, brought about by the acid, of such a nature as to increase the adhesion of the water and so delay its boiling." In other words, the glass was made chemically clean.   Still, however, the author does not catch this idea; he goes on contriving new experiments which again bring him " to some slight modification in the physical texture of the glass by the action of the sulphuric acid, or by destroying the impalpable dust retained between the molecules of the glass.   A new flask, however clean in appearance, always has more or less of such dust on its surface.   This kind of dust or varnish (*vernis*) not only adheres to the surface of glass, but forms part of its very substance, imprisoned between its molecules, and the adhesion of glass for water is diminished thereby."   Heating the glass to redness " may destroy foreign matter on the glass, or up to a certain point modify the molecular condition."   In the last page of his memoir Marcet is still referring to these " molecular changes " (so powerful was the influence of Gay-Lussac on scientific opinion) (10); and it is only in summing up the conclusions " drawn from his inquiry, still very incomplete (*encore fort incomplet*), that he makes use of the expression " perfectly clean glass vessels." In them, he says, " water and alcohol may be raised to a higher temperature than has hitherto been known."

20. In 1844 Magnus[*], in the course of some observations on the elasticity of steam, has some remarks on the cohesion and adhesion of water during the boiling.   If the walls of the vessel had an infinitely stronger adhesion for the water than the liquid particles had of cohesion among themselves, or if the walls of the vessel were of water, we might obtain a measure of the cohesive force, because in such case the water would only attain a boilingpoint at which the elasticity of the steam was sufficient to overcome the pressure of the air and the cohesion of the water. But in a vessel whose sides have less adhesion for the water than the liquid molecules have of cohesion among themselves, a less force is required to overcome the adhesion than the cohesion.   Hence the boilingpoint is lower in proportion as the adhesion of the sides or of any foreign insoluble substance in the water is small.   It is supposed that a smooth metal surface to which the water-particles adhere more strongly than they cohere, must raise the boilingpoint—although experiment shows the contrary.   If a perfectly clean metal surface be dipped into water so as to wet it com-

* Poggendorff's *Annalen*, vol. lxi. p. 248.

pletely, points will be found where the water does not adhere, where in fact the adhesion is less than the cohesion of the liquid molecules. The same is true of glass, though cleaned with boiling sulphuric or nitric acid. In a platinum vessel cleaned first with fused caustic potash and then with sulphuric acid, the boiling-point of water was higher than the temperature of the steam, but not so high as in a glass vessel; but in this the boiling-point was not so high as in Marcet's experiments (19). The reason probably was that the platinum vessel used was much worn, and contained cracks and scratches (*Risse und Schrammen*) which probably acted like pulverulent bodies in lowering the boiling-point.

21. In 1843 Donny published a paper* in which the influence of the air or of gases dissolved in the liquid is made of first-rate importance in studying the phenomena of ebullition. M. Donny's results have been estimated so highly that most subsequent writers have not only adopted them, but have promoted further investigations with reference to them. Whether it be true or not that the dissolved air is so important has been considered in another place†; but the idea that it is so is by no means new, as already noticed (7), (11), (14), &c.

22. According to Donny, boiling is not an inherent property of liquids; they only boil when they contain air—that is, when they are not pure. Heat liberates bubbles of air nearest to the source of heat: each air-bubble presents to the liquid molecules surrounding it a surface which promotes the vaporization of these molecules; and when the tension of the vapour is sufficient to counterbalance the pressure to which these bubbles are submitted, nothing further opposes the development of this vapour, which then forms currents that traverse the liquid and give rise to ebullition. Hence, according to this view, ebullition is a kind of evaporation extremely rapid, which operates upon those interior surfaces of the liquid which limit a bubble of some aëriform fluid‡.

23. If the quantity of air in the liquid be small, the boiling-point may rise; but the boiling-point is constant only when the liquid contains air. It is difficult, if not impossible, to get rid of the dissolved air; after a burst of vapour a bubble of air may often be seen adhering to the vessel. The boiling-point is also singularly influenced by the forces of cohesion and adhesion—the

* "Sur la Cohésion des liquides, et sur leur adhérence aux Corps solides," *Mémoires couronnés par l'Académie Royale de Bruxelles,* 1843 & 1844, vol. xvii. published 1845. See also *Annales de Chimie et de Physique* for 1846, ser. 3. vol. vi. p. 167.

† Proceedings of the Royal Society, January 21, 1869.

‡ These views are the same as those put forth by M. De Luc (7).

cohesion of the molecules of water being superior to a pressure of three atmospheres, or a column of water of 30 metres.   This conclusion was arrived at by heating water in a chemically clean tube in a bath of chloride of calcium as high as 138° C. (280°·4 F.).   The tube was 8 millims. in diameter ($\frac{3}{10}$ or $\frac{31}{100}$ inch), curved at an angle of 100° at the end where the water was heated, the other end being furnished with two bulbs.   The water was first boiled so as to expel the air, and the extreme bulb was then sealed.   The portion of the column of water contained in the short covered end was then put into the chloride-of-calcium baths, of which there were four, one at 113° C., another at 121°, a third at 128°, and a fourth at 132°, and this rose to 138°, at which temperature the water suddenly burst into steam and condensed in the bulbs at the other end of the tube.

24. From 1843 to 1863 it was considered a settled point in physico-chemistry that, in proportion as water is deprived of air, the character of its ebullition changes, becoming, as it does, more and more abrupt, and boiling, like sulphuric acid, with frequent *soubresauts*, and that between every two bursts of vapour the water reaches a temperature above its boiling-point.   To effect this, according to Mr. Grove[*], it is necessary that the water be boiled in a tube with a narrow orifice, through which the vapour issues ; if it be boiled in an open vessel, it continually reabsorbs air and boils in the ordinary way.   Mr. Grove describes an experiment in which water covered with oil was boiled in a tube : the water also contained some wire, which was platinized for the purpose of presenting more points for the ebullition and for preventing *soubresauts* as much as possible.   The boiling was continued for hours and even for days ; the steam condensed in the oil, and there was always found with it a minute bead of nitrogen gas.   It is contended that in this and another similar experiment there was no pure boiling of water, no rupture of cohesion of the molecules of water itself, but the water was boiled by evaporation against a surface of gas.   The conclusion is sufficiently startling, viz. "that no one has yet seen the phenomenon of pure water boiling, *i. e.* the disruption of the liquid particles of the oxyhydrogen compound so as to produce vapour which will, when condensed, be a water leaving no permanent gas."

25. In 1861 M. Dufour[†] published an account of an experiment in which water is said to have been raised to the temperature of 178° C. (352°·4 F.) without boiling.   For this purpose the water, previously raised to 80° or 90° C., was suspended in

* " On some Effects of Heat on Fluids," Journal of the Chemical Society for 1863, 2nd series, vol. i. p. 263.
† *Archives de la Bibliothèque Universelle de Genève.*   See also *Ann. de Chim. et de Phys.* for 1863, ser. 3. vol. lxviii. p. 378.

a mixture of oil of cloves and linseed-oil, and this was gradually raised to 110° or 115°; and the water did not burst into vapour unless it touched the sides of the vessel or the thermometer. The smallest globules of water bore the higher temperatures best: those of 10 millims. diameter were raised to 120° or 130° C.; while those of from 1 to 3 millims. were raised to 178°, when the vapour has an elastic force of 8 or 9 atmospheres *.

26. Solids brought into contact with the globules liberated vapour with a hissing noise. Porous bodies, such as wood, chalk, cotton, paper, &c., produced this effect best. A glass rod or a metallic wire did not always act in this way. A platinum wire by frequent use appeared to lose the power of causing sudden vaporization†. Porous bodies act best because they carry down air in which the globule begins to evaporate and expand.

27. M. Dufour rejects the theory which attributes the retardation in boiling to the adhesion of the liquid to the sides of the vessel. The contact of solids may prevent the liquid from rising above its boiling-point. The real explanation is to be found in the molecular relations of liquids—in a sort of internal cohesion. When a liquid is near the boiling-point, these molecular influences act as excitants to change of state. The adhesion to the sides of a vessel also excites a peculiar molecular condition in the liquid, and it is at the sides that this molecular equilibrium is disturbed and boiling takes place. But when the aqueous globule is immersed in a fluid with which water does not mix and is raised to a high temperature, it is the contact of a solid that disturbs the mechanical structure of the globule and induces change of state. Heat alone, acting on water protected from the air, contact of solids, and other disturbers of the molecular condition, cannot produce change of state, except very much above the temperature usually recognized as the boiling-point. But M. Dufour admits that the molecular influences or disturbers unfortunately present so many irregularities, that they have hitherto escaped the controlling action of any regular law. He thinks there must be some other force besides cohesion that

* I have no doubt that the globules were in the spheroidal state, as in De Luc's experiment (7, *note* *, p 165). More than thirty years ago I published in my 'Student's Manual of Natural Philosophy,' p. 553, an account of some experiments in which water, alcohol, ether, and some other liquids were gently delivered from a dropping-tube to the surface of a fixed oil heated to 450° or 500' F. The liquid drops rolled about on the surface in the spheroidal state; and in some cases, when a drop slipped beneath the surface, it exploded and scattered the oil about; but in other cases it was shot up again to the surface, where it continued to roll about as before.

† Dufour says in another part of his memoir that glass is less active as a promoter of vaporization than metal. " Il semble que le fréquent usage d'une pointe de platine contribue à l'amener à cette sorte de passivité."

delays the boiling of a liquid; " but these and numerous other phenomena which depend on mechanical molecularity are in a deplorable state of obscurity." He thinks that Gay-Lussac (8) uses the word *cohesion* in a wider sense than that which opposes the separation of particles. " We must simply conceive that the force which prevents the vapour from forming is an internal force due doubtless to cohesion of the liquid, which the vapour must overcome, and that resistance to change of state which it is more difficult to analyze."

28. In 1864 and the following year M. Dufour published two further memoirs* on the phenomena of boiling water, in which he shows that under certain conditions the retardation of the boiling-point takes place at reduced pressures and under atmospheres of hydrogen, street gas, and carbonic acid. As to the action of solids in promoting boiling, he has no doubt that it is due to the air adhering to them, and they become inactive when this air is removed by the boiling water—and that when the boiling-point of water rises by repeated boiling, the effect is due to the expulsion of air (11). Bits of dry pine-wood, paper, filaments of cotton, &c. lowered the boiling-point. " Soustraits depuis longtemps au contact de l'air, fréquemment et longuement chauffés dans l'eau, ils avaient fini par devenir absolument inactifs : jamais une bulle de vapeur ne se produisait plus sur leur surface, et des retards considérables d'ébullition pouvaient se manifester " (p. 210).

29. It will be seen from these historical notices that much importance is attached to the influence of dissolved air upon the boiling of liquids, as pointed out by De Luc (7) nearly a century ago, and more recently insisted on by Donny (21) and others. It is generally admitted in our text-books (1st) that as soon as this dissolved air has been expelled by heat, liquids boil with difficulty, or produce only sudden flashes of steam; (2nd) that those liquids which have only a weak affinity for air, such as sulphuric acid, alcohol, ether, &c., boil with the greatest difficulty ; (3rd) that the mutual cohesion of the molecules of the liquid, and the adhesion of the liquid to the sides of the vessel, influence the boiling-point, but the adhesion varies with the nature of the vessel and the condition of its sides as to roughness or smoothness ; (4th) that the action of solid substances in promoting tranquil boiling and in preventing *soubresauts* is by carrying down air.

30. My reasons for dissenting from these conclusions are given in my paper published in the Proceedings of the Royal Society (21, *note*), to which I beg to refer.

* *Archives des Sciences, Bib. Univ.* vol. xxi. p. 201 ; vol. xxiv.

XXIII. *On the Compounds of Ethylene-sodium and of its Homologues.* By J. ALFRED WANKLYN, *Professor of Chemistry in the London Institution*[*].

THE reaction described at the end of my former paper[†] indicates very distinctly that the absolute ethylate of sodium got by heating the well-known crystals is in reality *hydrated oxide of ethylene-sodium*, the substances arising from the action of the ethers on this compound being the different salts of the new organo-metal ethylene-sodium.

$((C^2 H^4)'' Na''')' =$ Ethylene-sodium (radical).

$\left.\begin{array}{l}(C^2 H^4 Na)' \\ \hspace{1.2em} H\end{array}\right\} O =$ Hydrated oxide of ethylene-sodium.

$\left.\begin{array}{l}(C^2 H^4 Na)' \\ C^2 H^3 O\end{array}\right\} O =$ Acetate of ethylene-sodium.

$\left.\begin{array}{l}(C^2 H^4 Na)' \\ C^5 H^9 O\end{array}\right\} O =$ Valerianate of ethylene-sodium.

$\left.\begin{array}{l}(C^2 H^4 Na)' \\ C^7 H^5 O\end{array}\right\} O =$ Benzoate of ethylene-sodium.

$((C^5 H^{10})'' Na''')' =$ Amylene-sodium (radical).

$\left.\begin{array}{l}(C^5 H^{10} Na)' \\ \hspace{1.5em} H\end{array}\right\} O =$ Hydrated oxide of amylene-sodium.

$\left.\begin{array}{l}(C^5 H^{10} Na)' \\ C^2 H^3 O\end{array}\right\} O =$ Acetate of amylene-sodium.

$\left.\begin{array}{l}C^5 H^{10} Na \\ C^5 H^9 O\end{array}\right\} O =$ Valerianate of amylene-sodium.

The foregoing is a list of the formulæ of such compounds of ethylene-sodium and of amylene-sodium as have been already produced, to which are added the formulæ of the radicals themselves.

Ethylene-sodium and Amylene-sodium (radicals).

It will of course be understood that should these radicals be capable of existing in an isolated state, their formulæ in that state must be double of that which represents them in a state of combination. Thus we should have :—

$\left.\begin{array}{l}(C^2 H^4 Na)' \\ (C^2 H^4 Na)'\end{array}\right\} =$ free ethylene-sodium.

Very great difficulties will have to be overcome before the free radicals can be actually prepared. Passing in review the history of the isolation of the organo-metallic radicals, it will be

[*] Communicated by the Author.
[†] Phil. Mag. vol. xxxvii. p. 117.

understood that the methods which have answered in the instances already known are not likely to answer in the present case. Kakodyle was obtained from chloride of kakodyle by driving out kakodyle by means of zinc. Supposing, however, that I had got the chloride of ethylene-sodium, what metal could I expect to be capable of driving out ethylene-sodium ?

Zincethyle was obtained by the distillation of the double zinc-compound of iodine and ethyle, when, as is well-known, zinc-ethyle distils over. The non-volatility of ethylene-sodium is a bar to the establishment of a parallel process.

The only hope which I have at present of isolating the new radical is the hope of finding it among the products of the destructive distillation of the double compound of sodium-ethyle and zinc-ethyle, not, however, in the distillate, but in the residue, along with the finely divided zinc and sodium which results from the destructive distillation in question.

With respect to the chemical constitution of ethylene-sodium, there are two modes of representation which will commend themselves to the chemical mind, viz. :—

$$((C^2 H^4)'' Na')', \text{ and } ((C^2 H^4)'' Na''')'.$$

Against the first, and in favour of the second formula (that to which I have given the preference) may be urged the fact that sodium appears to be a trivalent metal ; and also that the first method of representation would necessitate the assumption that in the hydrated oxide there existed oxygen not in direct union with sodium, whilst the second formula represents the sodium as directly combined with oxygen. Another reason for giving the preference to the second formula will be mentioned on a future occasion.

In support of the statement that sodium is a tri-valent metal, a statement which will be looked upon as a chemical heresy in certain quarters, I would bring forward the cases of the double zinc-sodium-ethyle and of sodium-triacetyle.

The analysis of the first of these compounds led to the empirical formula $(Na\, C^2 H^5 + Zn (C^2 H^5)^2)$ *, as will be seen on reference to my paper on the subject. It will also be remembered that I altogether failed to effect a separation between the sodium-ethyle and the zinc-ethyle. The real constitution of the compound is :—

$$\begin{array}{l} Na'''—C^2 H^5 \\ \quad\diagdown \\ \quad\quad C^2 H^5 \\ Zn''—C^2 H^5 \end{array}$$
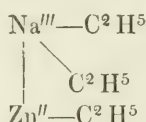
* Na $=23$, Zn $=65$.

Sodium-triacetyle, as I pointed out at the last Meeting of the British Association (*vide* also the January Number of Liebig's *Annalen*), is obtained by the action of sodium on acetic ether. Although its formula may be represented in another way, still the most elegant representation is $Na'''(C^2 H^3 O)^3$.

In the common sodium-salts I regard the sodium as having united with itself; thus common salt is looked upon as being

$$Na'''—Cl$$
$$||$$
$$Na'''—Cl$$
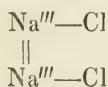
In fine, I regard sodium as being an analogue of nitrogen and arsenic rather than of hydrogen.

Hydrated Oxide of Ethylene-sodium.

This compound was described in my last paper under the name of the absolute ethylate of sodium, and is obtained by heating the well-known crystals which are the product of the action of sodium on alcohol. It is also formed by the action of sodium on the ethyle-ethers of the fatty acids.

It is a snow-white amorphous solid, non-fusible, and of remarkably low specific gravity. There are difficulties in the way of taking its specific gravity with great precision. It appears to be lighter than ether, in which it swims. There is just the possibility that this extreme lowness of specific gravity may be to some extent simulated, and that the floating in ether may be due to adherent gas (hydrogen).

Whether or not the specific gravity is lower than that of ether must be determined by further experiment; but that the specific gravity does not exceed that of water has been shown by a determination.

As has already been described, this substance possesses the property of withstanding a very high temperature without decomposition. It will bear being heated to 290° C.; possibly it will bear a much higher temperature; but, as might have been expected, it will not bear a low red heat without carbonizing.'

In contact with excess of water, it gives caustic soda and alcohol, the latter, as I showed in my last paper, being obtained in the theoretical quantity from product which had undergone heating to 200° C. Heated with an insufficient quantity of water to convert all of it into caustic soda and alcohol, the reaction is still of the same kind, a quantity of water liberating an equivalent of alcohol, thus:—

$$2 . \left.\begin{matrix} C^2 H^4 Na \\ H \end{matrix}\right\} O + 2 H^2 O = 2 C^2 H^6 O + Na^2 (HO)^2.$$
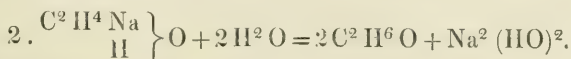
The numbers given on experiment were :—

Quantity of water added to a weighed quantity of the substance . . $\Big\}$ = 1·00 grm.

Loss on heating the substance and water to 270° C. and 280° C. . $\Big\}$ = 2·309 grms.

Therefore by experiment, ratio of water to alcohol produced
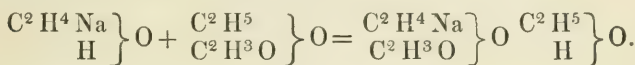
$$= 1·00 \,:\, 2·309.$$

Theory requires

$$1·00 \,:\, 2·55.$$

This experiment shows very plainly that there is no formation of anhydrous soda. If Na^2O were formed, double the quantity of alcohol should have been given. Furthermore, experiment with alcohol slightly moist, *i. e.* containing 3 per cent. of water, has shown that the new compound is very sensitive to the presence of traces of water, the smallest quantity of water effecting an equivalent decomposition according to the equation just given.

With iodide of ethyle :—It is well known that an alcoholic solution of potash or soda, as also the crystals containing alcohol and ethylate of sodium, effects an easy decomposition of iodide of ethyle into ether and iodide of the alkali-metal. The hydrate of ethylene-sodium behaves in a very different manner. I took 5·849 grms. of the hydrate and 10 cubic centims. of iodide of ethyle and heated very gradually up to 210° C. in a small retort open to the air; 9 cubic centims. of iodide of ethyle distilled over, and were condensed and measured. The product was subsequently weighed and found to be 5·948 grms., showing that there had been no sensible action, the small difference of 0·1 grm. being only that which would be occasioned by the difference of weight between the apparatus partially filled with vapour of iodide of ethyle and the apparatus filled with air. In this experiment, although the temperature of the oil-bath was 210° C., still the temperature of the interior of the retort must have kept very low, owing to the evaporation of the iodide of ethyle until near the end of the experiment. The conclusion to be drawn is that there is no rapid action between iodide of ethyle and hydrated oxide of ethylene-sodium at moderate temperatures. Under similar circumstances, either the crystals or alcoholic solution of potash or soda would have decomposed a very considerable quantity of iodide of ethyle.

When iodide of ethyle and the hydrated oxide of ethylene-sodium are sealed up together and heated up to between 120° C. and 150° C. for an hour or two, there is very complete decomposition.

1·25 grm. sodium was dissolved in alcohol, and the product dried at 196° C., 3 cubic centims. of iodide of ethyle added to it and sealed up with it. The whole was then heated to from 120° C. to 150° C. for about two hours. On opening the tube there was a slight escape of gas. Water was then added to the product, whereupon about 2 cubic centims. of light oil separated. This oil proved to be common ether, boiling at 36° C., and boiling to dryness at 40° C. The aqueous liquid was distilled, and the distillate redistilled. To the second distillate a little dry carbonate of potash was added, whereupon there formed two layers, viz. an aqueous layer of carbonate and an oil. This oil had the smell of the secondary alcohols, and was, I suppose, secondary butylic alcohol*.

With the ethers of the fatty acids there is produced alcohol and a salt of ethylene-sodium, *e. g.*

$$\left.\begin{array}{c} C^2\,H^4\,Na \\ H \end{array}\right\} O + \left.\begin{array}{c} C^2\,H^5 \\ C^2\,H^3\,O \end{array}\right\} O = \left.\begin{array}{c} C^2\,H^4\,Na \\ C^2\,H^3\,O \end{array}\right\} O \quad \left.\begin{array}{c} C^2\,H^5 \\ H \end{array}\right\} O.$$

A reaction of this kind has been observed with acetate of ethyle, acetate of amyle, valerianate of ethyle, and benzoate of ethyle. An analogous reaction has also been observed between the hydrated oxide of amylene-sodium and valerianate of amyle. There is every reason to believe that this reaction is perfectly general, applicable to the hydrated oxides of all the olefine compounds of sodium or potassium, and all the fatty and aromatic ethers.

Acetate of Ethylene-sodium.

As just mentioned, this salt is produced by the action of acetic ether on the hydrated oxide of ethylene-sodium. The reaction appears to take place slowly even at 100° C.† At higher

* Should further experiment confirm the indication here given, a very important step in organic synthesis will have been taken—from ethylic to isobutylic alcohol; and in like manner a regular ascent of the series should be practicable.
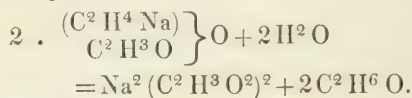
† Owing to the occurrence of this reaction at 100° C., the attempts made by various chemists to render acetic ether free from any alcohol with which it might be contaminated by distilling it off sodium, have had the reverse result, and, continually producing alcohol, have rendered the ether more impure. By the action of sodium on acetic ether there is produced sodium-triacetyle and hydrated oxide of ethylene-sodium, which latter attacks excess of acetic ether slowly at water-bath temperatures. A sample of acetic ether which, from the mode of its preparation, must have been originally almost, if not quite, free from alcohol, became, after several distillations off a small quantity of metallic sodium, charged with alcohol to the extent of 15 per cent. This circumstance, which really no one can be blamed for not expecting and providing against, has probably led Geuther, Frankland, and Duppa astray.

N 2

temperatures it proceeds more rapidly. At temperatures approaching to 200° C. it is rapid.

The most satisfactory way of preparing the compound appears to consist in sealing up hydrated oxide of ethylene-sodium with twice its weight of pure acetic ether, and heating for some time to 150° C. (at which temperature the materials form a clear fluid), and afterwards opening the tube and distilling off the alcohol and excess of acetic ether in the oil-bath.

It is very easy to effect a partial transformation of the hydrated oxide into the acetate, but not easy to complete the reaction.

Acetate of ethylene-sodium is a white solid, non-fusible at 200° C., but apparently readily soluble at 150° C. in acetic ether. Its characteristic reaction is the giving of alcohol and acetate of soda* by action on water.

$$2 \ . \ \left. \begin{array}{l} (C^2\,H^4\,Na) \\ C^2\,H^3\,O \end{array} \right\} O + 2\,H^2\,O$$
$$= Na^2\,(C^2\,H^3\,O^2)^2 + 2\,C^2\,H^6\,O.$$

It is isomeric with butyrate of soda. The circumstance of its furnishing alcohol and acetate of soda with water shows that the ethylene is *associated* with the metal and not with the acid part of the salt.

With iodide of ethyle it reacts in a very interesting manner. It combines with the iodide in the first instance, forming a solid. On the addition of water to this solid (which dissolves in the water, forming a solution of iodide of sodium), there separates a considerable quantity of oily liquid. This liquid does not contain any sensible quantity of common ether. I am engaged in the investigation of it.

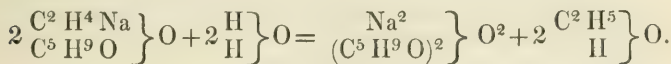### *Valerianate of Ethylene-sodium.*

Preparation.—Hydrated oxide of ethylene-sodium and pure valerianate of ethyle are heated in a retort placed in the oil-bath. Owing to the high boiling-point of valerianate of ethyle, it is unnecessary to make a preliminary digestion in sealed tubes, as in the instance of the acetate just described. The temperature of the oil-bath is to be gradually raised up to the boiling-point of valerianate of ethyle, and then taken up gradually to 200° C. During this operation abundance of alcohol has been

* The fact that this substance really does give alcohol and an acetate, and not the salt of any higher fatty acid, was established by treating some of it with water, and then distilling off the alcohol and estimating it in the distillate, and subsequently rendering the residue acid with dilute sulphuric acid and distilling. The distillate was then saturated with carbonate of baryta, and the resulting baryta-salt analyzed. It contained 53·34 per cent. of barium. Acetate contains 53·73.

observed to distil over; the valerianate of the ethylene-sodium remains behind as a white mass.

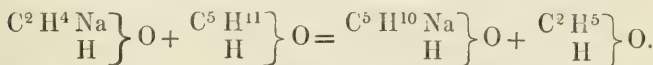Valerianate of ethylene-sodium gives alcohol and valerianate of soda with water, thus:—

$$2 \left. \begin{array}{c} C^2\,H^4\,Na \\ C^5\,H^9\,O \end{array} \right\} O + 2 \left. \begin{array}{c} H \\ H \end{array} \right\} O = \left. \begin{array}{c} Na^2 \\ (C^5\,H^9\,O)^2 \end{array} \right\} O^2 + 2 \left. \begin{array}{c} C^2\,H^5 \\ H \end{array} \right\} O.$$

Benzoate of Ethylene-sodium.

Formed like the salts just described. The high boiling-point of benzoic ether makes this a very convenient instance for the demonstration of the production of alcohol during these reactions. There is moreover no difficulty in effecting a tolerably complete transformation of a quantity of the hydrate into the benzoate. I subjoin the details of an experiment.

2·0555 grms. of sodium were dissolved in alcohol, and the resulting crystals heated up to 220° C., and maintained for some time between 220° C. and 230° C. Weight of the hydrate of ethylene-sodium = 5·528 grms. Added 17·455 grms. of pure benzoic ether and heated to 200° C. for about an hour, and just at last raised the temperature to 240° C. There distilled over a liquid, of which 2·9 grms. were collected and weighed. This proved to be absolute alcohol; it boiled to dryness sharply at 80° C. The weight of the solid residue was 19·486 grms., and the loss in weight by heating consequently was equal to 3·497 grms. The theoretical loss which the quantity of hydrated oxide of sodium-ethylene experimented on should have sustained is 4·111 grms., showing that in the above experiment the action had been nearly complete.

The benzoic ether used in the above experiment was quite pure, being neutral to test-paper, and having given correct numbers on titration with alkali.
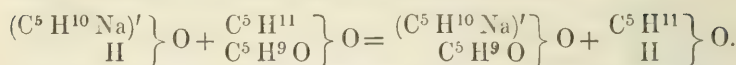
Hydrated Oxide of Amylene-sodium.

This compound is obtained like the corresponding ethylene-compound, viz. by the action of heat upon the product got on dissolving sodium in amylic alcohol. It is also formed in a very interesting way by the action of amylic alcohol on the hydrated oxide of ethylene-sodium:—

$$\left. \begin{array}{c} C^2\,H^4\,Na \\ H \end{array} \right\} O + \left. \begin{array}{c} C^5\,H^{11} \\ H \end{array} \right\} O = \left. \begin{array}{c} C^5\,H^{10}\,Na \\ H \end{array} \right\} O + \left. \begin{array}{c} C^2\,H^5 \\ H \end{array} \right\} O.$$

The salts of amylene-sodium are parallel with those of ethylene-sodium. The acetate and valerianate have been prepared; the latter is particularly distinguished from the ethylene-sodium salts by being readily fusible. In the preparation of these salts

amylic alcohol is the complementary product, thus :—

$$\left.\begin{array}{c}(C^5\,H^{10}\,Na)' \\ H\end{array}\right\} O + \left.\begin{array}{c}C^5\,H^{11} \\ C^5\,H^9\,O\end{array}\right\} O = \left.\begin{array}{c}(C^5\,H^{10}\,Na)' \\ C^5\,H^9\,O\end{array}\right\} O + \left.\begin{array}{c}C^5\,H^{11} \\ H\end{array}\right\} O.$$

Like the corresponding ethylene-compounds, these salts of amylene-sodium react on water, giving amylic alcohol and a common soda-salt of the acid they contain.

Some of the most striking and easiest to verify of the experimental facts of which the foregoing is the scientific expression are the following :—

(1) That hydrated oxide of ethylene-sodium (absolute ethylate of sodium) and different ethers of the fatty and aromatic acids furnish alcohol on being heated to about $200°$ C.

(2) That the weight of the product obtained on heating these materials is much less than the weight of one molecule of hydrated oxide of ethylene-sodium and one molecule of the ether of the fatty acid.

(3) That *all* the alcohol radical employed, both in the state of hydrated oxide of ethylene-sodium or hydrated oxide of amylene-sodium, and in the state of ether of fatty or aromatic acid, is to be recovered after the reaction in the state of alcohol—alcohol distilling over before, and alcohol distilling over after the addition of water to the solid product.

(4) That the fatty acid found after the reaction is the same as that employed at the beginning.

With regard to (1), it will be comprehended that by the use of an absolute ethylate charged with some caustic soda, arising from moisture in the alcohol, or by the employment of an ethylate containing alcohol in combination (such as the imperfectly decomposed crystals), the production of alcohol would be simulated. The author of this paper has specially guarded against these sources of fallacy, and has made sure that more alcohol is given than could be accounted for in any such manner.

XXIV. *Fundamental Principles of Molecular Physics.* *By* Professor J. Bayma, S. J., *of Stonyhurst College.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

IN the last Number of the Philosophical Magazine (February 1869, pp. 98–111) I find an article by Professor W. A. Norton, entitled "Fundamental Principles of Molecular Physics," intended to be an answer to certain remarks which I made in my 'Elements of Molecular Mechanics' (pp. 186–189), on his theory of molecular physics. My remarks were designed to show that some of the principles of the learned Professor did

not rest upon sufficient proof, and contained a good deal of arbitrary assumption. Professor Norton, of course, is not of my opinion, and argues that my objections "are destitute of any real force." This was his undoubted right ; and, had he confined himself to the defensive, I should have deemed a reply unnecessary ; but, as he goes on to attack my own theory with arguments which have a plausible appearance, I think it worth while submitting his article to examination.

The contents of his ably written paper may be summarized, as it appears to me, in the three following propositions :—

1st. That in molecular science all our general principles are but probable hypotheses.

2nd. That his own principles are not arbitrary assumptions.

3rd. That I in my ' Elements of Molecular Mechanics ' assume principles destitute of sufficient proof.

To these three propositions I beg leave to offer a short reply, which, I hope, will be of interest to all who are engaged, either directly or indirectly, in promoting molecular science.

I.

And to begin with the first proposition, Professor Norton says :

" No theory of molecular physics can, in the nature of things, have any other foundation than general principles to be regarded as hypotheses that have been rendered more or less probable, either by induction from observation, or by *à priori* reasonings. Molecular physics cannot be erected, like mathematics, upon a foundation known to be eternally sure, that of self-evident truth."

These words express an opinion, which is by no means un- common among physicists, for the obvious reason that they cannot, by the help of physics alone, undertake to establish fundamental principles ; a work, which requires a higher know- ledge of speculative science, than it is the fashion for them to acquire. Hence I do not wonder that mere physicists, even with their hands full of good scientific materials, can build only hypothetical theories. The cement, which alone can keep together the stones of a scientific building, is an article which cannot be prepared by the skill of the chemist, but only by the application of general philosophy to experimental truth ; and only in proportion as physicists are conversant with general philosophy can they hope to build theories of a character truly scientific, viz. independent of hypotheses ; for hypothesis begins there only, where real science ends. I do not wish to develope this point at greater length. I am satisfied that this mere hint will suffice for those whose judgment is of any weight in matters of this sort. They will see at once that I had good reasons for

saying ('Molecular Mechanics,' p. 2) that *a work which was intended to lay down the very first foundation of a molecular theory, could not possibly rest on hypothetical ground*, and that *I had embodied in the work, as principles of molecular mechanics, those propositions only which were evident, or which I thought I had the power of rigorously demonstrating from known laws of nature*; a proposition which seems to have amazed Professor Norton. But even he will allow, I hope, that, in my capacity of Professor of Philosophy, I can aspire to something better than those hypotheses which are the lot of mere experimentalists.

And now to come to his assertion. I would observe in the first place that, if " no theory of molecular physics can have any other foundation than general principles to be regarded as hypotheses," then no physical theory can lead to any other than hypothetical conclusions; and as hypothetical conclusions cannot be called " established truths" in any true sense of the words, it follows that neither Professor Norton's theory nor its principles are " established truths." And this would suffice to justify my stricture that his theory rests upon " a great deal of arbitrary assumption."

But I must add secondly, in justice to molecular science, that Professor Norton's opinion of it is far from being accurate. Molecular science is *not* without established principles : it is *not* a pure heap of hypotheses. That material substance is endowed with active power, passivity, and inertia, for causing, receiving, and conserving local motion : that no natural cause can communicate a finite velocity to a body in an infinitesimal unit of time : that in the impact of bodies no communication of motion can be made by means of a true and immediate contact of matter with matter : that an increase or decrease of intensity in motion is always due to a real production or extinction of velocity : that material substance acts, *cæteris paribus*, with different intensity on bodies placed at different distances : that material substance is not prevented by intervening bodies from acting on other bodies placed at a greater distance : that the primitive components of molecules are unextended elements : that these primitive elements obey the Newtonian law of action : and other points of the like nature, with all their corollaries, are not mere hypotheses, but " established truths," about which I think that the readers of the ' Elements of Molecular Mechanics ' can entertain no doubt. And whilst we must admit that we cannot by means of these truths alone attain to a knowledge of all the secrets of molecular science, we do nevertheless affirm that we have in them a solid foundation to build upon. So false is it that molecular science has no established principles, that even Professor Norton's theory, of which some fundamental prin-

ciples are, in my opinion, not sufficiently probable, contains undoubtedly a great deal of " established truth."

But "molecular physics," says he, "cannot be erected, like mathematics, upon a foundation known from the first to be eternally sure, that of self-evident truth." This remark of my learned critic does not show that molecular physics cannot be erected on *certain* truth, but only that physical science, whether molecular or not, does not deal, like mathematics, with truths which consist of necessary relations, but with truths dependent upon contingent facts. The first are known mainly by reason, the secondly mainly by observation : and no one demurs to *physical* certainty on the ground that it is not *mathematical.*

Professor Norton adds :

" Mechanical axioms may exist as mere figments of the mind, and have often risen like bubbles in the minds of speculative philosophers, shone with an evanescent splendour, and suddenly burst at the touch of a hard fact."

I do not deny that axioms may exist as mere figments in the minds of " speculative " philosophers ; but the learned Professor has surely forgotten that all philosophers, who build theories, are " speculative " philosophers. Otherwise how is it possible that he should write a theory of molecular physics, and yet be so hard on speculative philosophers amongst whom his work places him ?

But Professor Norton apparently wishes to designate as " speculative philosophers " a particular school of philosophers, whose speculations have their origin in *à priori* reasonings instead of natural facts. If so, I may safely reply that such a school (to which he imagines me to belong) has never existed. There has indeed been a school of philosophers, whose physical speculations are now known to have been in most instances illustrious failures ; nevertheless, that school notoriously abided by facts ; and their failure was caused by misinterpretation of facts, not by any desire of substituting *à priori* grounds for *à posteriori* arguments. In what does modern speculation differ from that of the ancients ? We possess, to be sure, a greater number of experimental data ; but this advantage (which does not change the nature of scientific speculation) is frequently counterbalanced in a great measure by the vagueness and shallowness of our philosophical acquirements. Facts and laws are only materials for speculation : something more is needed for building sound physical theories : and this *something* is not so much the power of imagining new hypotheses, as the art of reasoning and an intimate acquaintance with those speculative principles which apply to the interpretation of facts. Such principles are the

supreme test of scientific hypotheses : and if our men of science were less afraid of speculative knowledge than they appear to be, we should be less familiar with the sight of " bubbles shining with an evanescent splendour, and suddenly bursting at the touch of a hard fact," or, let me add, of a still harder principle.

Professor Norton thus continues :

" Our author is another instance of a learned philosopher, who has faith in such unsubstantialities, and thinks to substitute them, as a proper basis for a theory of molecular mechanics, in place of the general conceptions, to which the progress of science leads, and by which alone its highest inductions find any explanation—regards the latter as arbitrary assumptions, and his own mental convictions of what matter must be and how it must act as the only reliable foundation upon which to build."

This passage is made up of assertions which may easily be retorted against my critic. But to answer directly. The progress of science leads without doubt to general conceptions, which are sometimes very good, and at other times very questionable, according as those who deal with the matter are more or less conversant with the principles of speculation and the highly important art of reasoning. But the question lies not in this : it lies in the *truth* or *falsehood* of the assertion that I " substituted " unsubstantialities for those general conceptions. Now, I think that everyone who has read my ' Elements of Molecular Mechanics ' can bear witness to the gratuitousness of the assertion. What Professor Norton calls " unsubstantialities " (probably because he cannot touch them with his finger) are considerations which have not been *substituted* in the place of the general conceptions to which the progress of science leads, but have been *added*, for the greater satisfaction of a class of readers, under the form of *scholia*, to the theorems by which those general conceptions are shown to be legitimate results of the progress of science : and have been appended not to prove those theorems (which had no need of a second demonstration) but to meet the " unsubstantialities " of some metaphysicians, who are yet to be reconciled with modern science in certain matters : and, lastly, they have been printed in a smaller type, *lest the reader should mistake them for the substance of the work, and engage unawares in the awful mysteries of philosophical speculation* (Introd. p. 6). Let Professor Norton read again the first pages of my work : I do not doubt but that he will discover his mistake.

He states likewise that I regard those general conceptions (to which the progress of science leads) as arbitrary assumptions, and my own mental convictions as the only reliable foundation upon which to build. Surely, Professor Norton himself relies

upon his own mental convictions, and considers as arbitrary assumptions those conceptions which clash with them : and he may easily understand that I cannot but do the same. But such mental convictions as he attributes to me are quite impossible ; for if a man, in matters dependent on facts and laws of nature, should disregard " the general conceptions to which the progress of science leads," how could he ever find a ground on which to rest his mental convictions ?

The truth is that Professor Norton has failed to realize to himself the spirit in which my ' Elements of Molecular Mechanics ' were written. He is mistaken in his assumption that I virtually claimed for my method a superiority which he is not ready to recognize. The geometrical method, which I adopted, has its own advantages, independently of the writer who employs it, as everyone must allow : but I was so far from claiming any superiority for the method *as employed in my work*, that I explicitly declared the contrary. *As the employment of the geometrical method may have given to the work an air of dogmatism in questions regarding which there are great differences of opinion among philosophers, I beg to say once for all that I have merely stated my own views, without pretending to make further discussion unnecessary* (p. 10). Professor Norton has apparently failed to notice these words.

But the whole passage in which I am thus attacked deserves to be here inserted :

" It is true that he takes exceptions to Principles 3rd and 4th from the inductive point of view. Upon this ground (the only legitimate one to be occupied) I am quite ready to meet him : but I wish to enter here, at the outset, a demurrer against the virtual claim of the superiority of his own *à priori* method of establishing his fundamental principles. Such a claim is implied in the intimation that ' no one has up to this day established the truth of such propositions ' as will be best appreciated by those who have read Professor Bayma's book."

My American critic acknowledges that I argued against two of his principles " from the inductive point of view." Hence my intimation that no one had yet established the truth of such propositions, was obviously drawn " from the inductive point of view." How could then my learned critic construe it into a virtual claim of the superiority of any *à priori* method ?

The learned Professor shows a great hostility to what he rather invidiously styles " my *à priori* method." But a method in which arguments are presented under a syllogistic form, is not necessarily an *à priori* method. Professor Norton seems to remember only two methods, the *à priori* and the *inductive*. He seems to have forgotten that there is a third, the *à posteriori*

and deductive, which consists of reasoning based on observation. This is the method which I followed in establishing the fundamental principles of molecular mechanics. The *à priori* method would have been absurd. Professor Norton may consequently show his horror of the *à priori* method as much as he likes: after all, it is only a matter of taste, about which I am not concerned.

He continues:

" Having proved, as he conceives, his propositions, and clinched each one of them with a Q. E. D., he insists that obvious intimations of nature are to be discarded, because the stamp of infallibility cannot be put upon them at once, before the test of availability in the explanation of phenomena has been applied."

This language is rather severe. Let the learned Professor show, if he can, in the first place, that I have not sufficiently proved my fundamental propositions. I should consider it a great favour, as I love nothing more than truth. Secondly, let him bring forward, if he can, a single passage in my ' Elements of Molecular Mechanics' to establish his statement that " I insist that obvious intimations of nature are to be discarded." As for the " stamp of infallibility " I need not say that it is a merry invention of my American critic.

He then adds:

" It was evident from the tenor of my exposition of the subject, that ' the established truths ' referred to were merely regarded as having been virtually established, or rendered highly probable by the inductions of science. The claim implied in Professor Bayma's criticism, that they require a higher confirmation, in fact a demonstration of their truth, is not to be admitted."

I was perfectly well aware that Professor Norton regarded his 3rd and 4th Principles as having been only virtually established. But, as to the fact itself, I was not of his opinion. Hence the claim implied in my criticism was not precisely that those principles " required a higher confirmation, in fact a demonstration of their truth," but that they had yet to be " virtually established " in the sense in which Professor Norton uses the word. I adduced reasons to prove this point. Unless my critic succeeds in answering those reasons (as he endeavours to do in the next part of his paper), I maintain that my claim is too reasonable not to be admitted.

[To be continued.]

XXV. *On the Theory of Sound.   By* R. Moon, *M.A., Honorary Fellow of Queen's College, Cambridge*[*].

THE transmission of sound through air confined within a cylindrical tube, when the axis of the tube is the direction of transmission, may be represented by a single partial differential equation, which in its rudest form is of a very simple character.

If, when the air is undisturbed, x denote the distance from the origin of an element of the thickness dx made by planes perpendicular to the axis, if y be the same distance at the time t after disturbance, p the pressure at the time t at the point whose ordinate is y, and D denotes the density of equilibrium, then, the pressure at the time t on the one side of the element being p and on the other side being $p + \dfrac{dp}{dx} dx$, the moving force on the element, estimated without reference to sign, will be $\dfrac{dp}{dx} dx$; and dividing this last by Ddx, the mass of the element, we get $\dfrac{1}{D} \cdot \dfrac{dp}{dx}$ for the *accelerating* impressed force acting on the element at the time t.

The corresponding effective force on the element will of course be $\dfrac{d^2y}{dt^2}$; hence equating these in accordance with D'Alembert's principle, account being now taken of signs, we get for the differential equation of motion,

$$0 = \frac{d^2y}{dt^2} + \frac{1}{D} \cdot \frac{dp}{dx}. \quad . \quad . \quad . \quad . \quad . \quad (1)$$

If we assume that Mariotte's law, which has been proved to hold very approximately in the case of equilibrium, holds also when the air is in motion (observing that $\dfrac{D}{\rho} = \dfrac{dy}{dx}$, and therefore $p = a^2\rho = a^2 . D . \overline{\dfrac{dy}{dx}}\Big|^{-1}$, where ρ denotes the density), (1) becomes

$$0 = \frac{d^2y}{dt^2} - a^2 \overline{\frac{dy}{dx}}\Big|^{-2} \cdot \frac{d^2y}{dx^2}. \quad . \quad . \quad . \quad . \quad (2)$$

When the motions are small, (2) reduces to

$$0 = \frac{d^2y}{dt^2} - a^2 \frac{d^2y}{dx^2}, \quad . \quad . \quad . \quad . \quad . \quad (3)$$

from which approximate equation the theoretical value of the

* Communicated by the Author.

velocity of sound has been calculated to be 916·322 feet per second*, the true velocity being about one-fifth greater.

The discrepancy thus evinced between the results of theory and of experiment was long supposed to have been removed by the celebrated correction of (3) on account of temperature proposed by Laplace, by means of which the theoretical and the ascertained values of the velocity of sound were conceived to have been brought within a very small fraction of each other.

A most competent judge† has pronounced, however, that the experimental processes of Clément and Desormes, and of Gay-Lussac and Welter, upon which this supposed coincidence has been founded, are worthy of no confidence whatever ; " de sorte que l'explication de Laplace n'est encore aujourd'hui qu'une hypothèse, très-ingénieuse sans doute, mais qui a besoin d'être confirmée par l'expérience."

A further and decisive objection may be taken also to Laplace's correction in the form in which it is actually presented to us on the following ground.

The correction depends upon the assumption that "for very small condensations the rise of temperature will be proportional to the increase of density " (*Encyc. Met.* No. 72).

Now if τ denote the excess of temperature at the time t above the mean temperature, it is clear that in any case of motion the value of τ at a given time and place must be known, *i. e.* we must have

$$\tau = \text{funct. } (y, t) ;$$

and if s denote the condensation and v the velocity at the time t at the point whose ordinate is y, we shall have in like manner

$$s = \text{funct. } (y, t),$$

$$v = \text{funct. } (y, t).$$

Hence, eliminating y and t between the last three equations, we have

$$\tau = \text{funct. } (s \cdot v) ;$$

and since it is evident that τ will not become infinite when either s or v vanishes, we are warranted in concluding that, when the motions are small,

$$\tau = A \cdot s + B \cdot v,$$

where A and B are constants ; and we are *not* warranted in assuming that $\quad \tau = A \cdot s‡,$

as in effect has been done by Laplace and Poisson.

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* *Encyc. Met.* art. Sound, No. 66.

† M. Regnault, see *Mém. de l'Acad.* vol. xxiv. p. 40 (1862).

‡ The comparative success which had attended the bold hypothesis of the uniformity of the law of pressure in the cases of motion and of equili-

But the objections to the principle upon which equations (2) and (3) have been obtained are not confined to the failure of the latter equation to give a calculated velocity of sound reconcilable with the experimental velocity.

The principle in question, viz. that the equation $p = a^2\rho$ holds in all cases irrespective of the state of rest or motion of the fluid, is a pure assumption, having no basis of fact or argument to rest upon. This will appear most clearly as follows.

In any case of motion under the circumstances we are considering, the pressure at a given point at a given time must be known: the same may be said of the velocity; the same of the density; so that we must have

$$p = \text{funct. } (y, t),$$
$$v = \text{funct. } (y, t),$$
$$\rho = \text{funct. } (y, t).$$

Eliminating y and t between these three equations, and solving the result with respect to p, we shall get

$$p = \text{funct. } (v, \rho). \quad \quad \ldots \quad \ldots \quad (4)$$

It is evident that this last equation affords no more proof of the truth under all circumstances of the equation

$$p = a^2\rho \quad \ldots \quad \ldots \quad \ldots \quad \ldots \quad (5)$$

than the fact that the series

$$A + A_1 x + A_2 x^2 + \&\text{c. } \textit{in inf.}$$

becomes A when $x = 0$ affords that the same series has the value A whatever be the value of x.

If the substitution for the general equation (4) of its particular case (5) enabled us to escape from difficulties which otherwise are insurmountable, the continuing to adopt such substitution could be readily understood. The truth is, however, that the exact contrary is the fact.

All efforts hitherto have failed to elicit a solution of the problem of adequate generality from (2), which is what (1), the true indisputable equation of motion, becomes when (5) is taken to represent the law of pressure, the most general solution hitherto so obtained (that of Poisson) involving only one arbitrary

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brium naturally tempted Laplace to hazard the corresponding and equally unwarranted assumption with regard to the temperature. At a time like the present, when the principles of the mechanical theory of heat are generally diffused and recognized, a suggestion that the law of temperature in a fluid is irrespective of the fluid's state of rest or motion could hardly be entertained.

function*.   On the other hand, I have elsewhere † shown that, adopting for the pressure the only expression which the facts of the case authorize us to assume (that, namely, embodied in (4)), a solution can be obtained possessing a degree of generality not merely equal to all that had been considered requisite, but even greater than had been conceived to be attainable.

The solution to which I refer is contained in the three following equations, viz. :—

$$
\left.
\begin{aligned}
p + \frac{\alpha^2}{\rho} &= \chi\left(v \pm \frac{\alpha}{\rho}\right), \\
v \pm \frac{\alpha}{\rho} &= \phi\left\{ x - \frac{\chi' \mp \alpha}{D} \cdot t \right\}, \\
\frac{1}{\rho} + f\left(v \pm \frac{\alpha}{\rho}\right) &= \psi\left\{ x \mp \frac{\alpha}{D} \cdot t \right\},
\end{aligned}
\right\}
\quad \cdots \quad (6)
$$

where α is an arbitrary constant; χ, ϕ, and ψ are arbitrary functions; and the form of f is defined by the equation

$$
f(u) = \int \frac{du}{\chi'(u) \mp 2\alpha}.
$$

For the details of the method by which this result has been obtained I must refer to my former paper.   Its general character may be described as follows.

Equation (4) enables us to put (1) under the form

$$
0 = \frac{d^2y}{dt^2} + \frac{1}{D}\left(\frac{dp}{d\rho} \cdot \frac{d\rho}{dx} + \frac{dp}{dv} \cdot \frac{dv}{dx}\right);
$$

or, since $v = \dfrac{dy}{dt}$ and $\rho = D \cdot \left.\dfrac{dy}{dx}\right.^{-1}$,

$$
0 = \frac{d^2y}{dt^2} + \frac{1}{D} \cdot \frac{dp}{dv} \cdot \frac{d^2y}{dx\,dt} - \frac{dp}{d\rho}\left.\frac{dy}{dx}\right.^{-2}\frac{d^2y}{dx^2}. \quad \cdots \quad (7
$$

Regarding (7) simply as a partial differential equation between $x, t, y, \dfrac{dy}{dx}, \dfrac{dy}{dt}$ requiring solution, the most obvious mode of treating it is to attempt its solution by Monge's method.   I have shown that, in order that (7) may admit of two integrals of the kind given by Monge's method, two equations of condition must be satisfied involving p, ρ, and v only; *both* of which are satisfied by the relation between p, ρ, and v contained in the first of equations (6).   When that relation holds, (7) will have for first integrals the last two equations of the same group.

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* It would be easy to point out the cause of this failure.   Upon this branch of the subject, however, I do not propose to enter at present.

† See the Philosophical Magazine for August 1868.

That equations (6) constitute a true solution of the problem (and therefore *the* true solution unless the possibility be entertained of there being other solutions possessed of the requisite degree of generality) may be verified as follows.

Eliminating ϕ from the second of equations (6) by differentiation in the usual manner, we get

$$0 = \frac{dv}{dt} \mp \frac{\alpha}{\rho^2} \frac{d\rho}{dt} + \frac{\chi' \mp \alpha}{D} \left(\frac{dv}{dx} \mp \frac{\alpha}{\rho^2} \frac{d\rho}{dx} \right). \quad . \quad . \quad . \quad (8)$$

The first of equations (6) gives us

$$\frac{dp}{dx} = \frac{\alpha^2}{\rho^2} \frac{d\rho}{dx} + \chi' \left(\frac{dv}{dx} \mp \frac{\alpha}{\rho^2} \frac{d\rho}{dx} \right)$$

$$= \pm \alpha \frac{dv}{dx} + (\chi' \mp \alpha) \left(\frac{dv}{dx} \mp \frac{\alpha}{\rho^2} \frac{d\rho}{dx} \right). \quad . \quad . \quad . \quad . \quad (9)$$

We have also the analytical condition

$$\frac{d}{dx} \left(\frac{dy}{dt} \right) = \frac{d}{dt} \left(\frac{dy}{dx} \right) ;$$

or, since $v = \dfrac{dy}{dt}$ and $\dfrac{dy}{dx} = \dfrac{D}{\rho}$,

$$\frac{dv}{dx} = - \frac{D}{\rho^2} \frac{d\rho}{dt}.$$

Substituting this value of $\dfrac{dv}{dx}$ in (9), it becomes

$$\frac{dp}{dx} = \mp D \frac{\alpha}{\rho^2} \frac{d\rho}{dt} + (\chi' \mp \alpha) \left(\frac{dv}{dx} \mp \frac{\alpha}{\rho^2} \frac{d\rho}{dx} \right).$$

Dividing this last equation by D and subtracting from (8) we get

$$0 = \frac{dv}{dt} + \frac{1}{D} \frac{dp}{dx},$$

which is identical with (1), the true equation of motion.

The same mode of treatment, with the same result, may be applied to the third of equations (6).

I shall now proceed to draw certain conclusions from the foregoing formulæ in the case where the motions are small.

The first of (6) gives us

$$p = - \frac{\alpha^2}{\rho} + \chi \left(v \pm \frac{\alpha}{\rho} \right)$$

$$= \pm \alpha v \mp \alpha \left(v \pm \frac{\alpha}{\rho} \right) + \chi \left(v \pm \frac{\alpha}{\rho} \right)$$

$$= \pm \alpha v + \pi \left(v \pm \frac{\alpha}{\rho} \right),$$

suppose, where π denotes an arbitrary function.   Therefore

$$\frac{dp}{dv} = \pm \alpha + \pi'\left(v \pm \frac{\alpha}{\rho}\right),$$

$$\frac{dp}{d\rho} = \mp \frac{\alpha}{\rho^2}\pi'\left(v \pm \frac{\alpha}{\rho}\right);$$

and substituting these values in the equation of motion (7), we get

$$0 = \frac{d^2y}{dt^2} + \frac{1}{D}\cdot\left\{\pi'\left(v\pm\frac{\alpha}{\rho}\right)\pm\alpha\right\}\frac{d^2y}{dx\,dt} \pm \frac{\alpha}{\rho^2}\cdot\pi'\left(v\pm\frac{\alpha}{\rho}\right)\frac{dy}{dx}\bigg|^{-2}\frac{d^2y}{dx^2};$$

and putting $v=0$ and $D=\rho$ in the coefficients of $\dfrac{d^2y}{dx\,dt}$, $\dfrac{d^2y}{dx^2}$, as

we may do when the motions are small, we get, observing that $\dfrac{dy}{dx} = \dfrac{D}{\rho}$,

$$0 = \frac{d^2y}{dt^2} + \frac{1}{D}\cdot\left\{\pi'\left(\frac{\alpha}{D}\right)\pm\alpha\right\}\frac{d^2y}{dx\,dt} \pm \frac{\alpha}{D^2}\cdot\pi'\left(\frac{\alpha}{D}\right)\cdot\frac{d^2y}{dx^2}. \quad (10)$$

If we consider the case of a disturbance confined to a portion of the tube defined by planes perpendicular to the axis, and in which the law of continuity is preserved (that is, which offers no sudden changes of velocity or density), it is clear that the values of p at the boundaries of the disturbance will follow Mariotte's law; so that we shall have

$$\pi\left(\pm\frac{\alpha}{D}\right) = a^2 \cdot D = \pm a^2\alpha\left(\pm\frac{\alpha}{D}\right)^{-1}$$

and

$$\pi'\left(\pm\frac{\alpha}{D}\right) = \mp a^2\alpha\left(\pm\frac{\alpha}{D}\right)^{-2}$$

$$= \mp \frac{a^2D^2}{\alpha}.$$

Substituting this value, (10) becomes

$$0 = \frac{d^2y}{dt^2} \pm \left(\frac{\alpha}{D} - a^2\cdot\frac{D}{\alpha}\right)\frac{d^2y}{dx\,dt} - a^2\frac{d^2y}{dx^2},$$

where a and D are known and α is an unknown constant; or we get, finally, for the approximate equation of motion,

$$0 = \frac{d^2y}{dt^2} \pm 2ae\,\frac{d^2y}{dx\,dt} - a^2\frac{d^2y}{dx^2}, \quad . \quad . \quad . \quad (11)$$

where e is a constant the value of which must be determined by experiment.

We have here, in effect, two equations of motion*, of which the first, viz.

$$0 = \frac{d^2y}{dt^2} + 2ae\frac{d^2y}{dx\,dt} - a^2\frac{d^2y}{dx^2} \quad . \quad . \quad . \quad (12)$$

has for its general integral

$$y = \phi\{x - a(e + \sqrt{1+e^2})t\} + \psi\{x - a(e - \sqrt{1+e^2})t\} \,;$$

or if ae be small in comparison with a,

$$y = \phi\{x - a(1+e)t\} + \psi\{x + a(1-e)t\} \,;$$

whence we have

$$\left.\begin{aligned}
\frac{dy}{dt} &= -a(1+e)\phi'\{x - a(1+e)t\} + a(1-e)\psi'\{x + a(1-e)t\} \\
\frac{dy}{dx} &= \phi'\{x - a(1+e)t\} + \psi'\{x + a(1-e)t\}.
\end{aligned}\right\} . \quad (13)$$

If $F(x)$, $f(x)$ be the respective values of $\frac{dy}{dt}$, $\frac{dy}{dx}$ when $t = 0$, we shall have

$$F(x) = -a(1+e)\phi'(x) + a(1-e)\psi'(x),$$
$$f(x) = \phi'(x) + \psi'(x) \,;$$

whence we get

$$\phi'(x) = -\frac{1}{2}\left\{\frac{1}{a}F(x) - (1-e)f(x)\right\},$$
$$\psi'(x) = \frac{1}{2}\left\{\frac{1}{a}F(x) + (1+e)f(x)\right\}.$$

Substituting these values in (13), and neglecting terms involving e^2, we get

$$v = \frac{dy}{dt} = \frac{1}{2}\left[\begin{aligned} &(1+e)F\{x - a(1+e)t\} - af\{x - a(1+e)t\} \\ &+ (1-e)F\{x + a(1-e)t\} + af\{x + a(1-e)t\} \end{aligned}\right],$$

$$\frac{y}{dx} = \frac{1}{2}\left[\begin{aligned} &-\frac{1}{a}F\{x - a(1+e)t\} + (1-e)f\{x - a(1+e)t\} \\ &+ \frac{1}{a}F\{x + a(1-e)t\} + (1+e)f\{x + a(1-e)t\}. \end{aligned}\right]$$

Suppose that when $t = 0$ the disturbance is confined to the length l measured from the origin to the right, then

$$F(x) = 0, \text{ if } x > l \text{ or } < 0,$$
$$f(x) = 1.$$

* Whether of the two is to be adopted as representing the motion in any particular case will hereafter be explained.

If x be measured positively to the right, we shall have at all points to the right of the original disturbance

$$F\{x+a(1-e)t\}=0, \quad f\{x+a(1-e)t\}=1,$$

whatever be the value of t; so that for all points to the right of the original disturbance the motion will be represented by

$$v= \frac{1+e}{2} F\{x-a(1+e)t\} + \frac{a}{2}[1-f\{x-a(1+e)t\}], \quad (14)$$

$$\frac{dy}{dx}=- \frac{1}{2a} F\{x-a(1+e)t\} + \frac{1}{2}[1+e+(1-e)f\{x-a(1+e)t\}].(15)$$

Moreover, for all points to the right of the original disturbance we shall have

$$F\{x-a(1+e)t\}=0, \quad f\{x-a(1+e)t\}=1$$

for all values of t between zero and that value of t which gives

$$x-a(1+e)t=l,$$

i. e. between $t=0$ and $t=\dfrac{x-l}{a(1+e)}$; in other words, at a point x to the right of the original disturbance there will be neither velocity nor condensation until t has attained the latter value, at the same time that when t has that value the velocity and condensation immediately begin to have significance.

It follows, therefore, that the disturbance has taken the time $\dfrac{x-l}{a(1+e)}$ to traverse the space $x-l$, having been propagated to the right with the velocity $a(1+e)$.

Multiplying (14) by $1-e$, and neglecting terms in the result involving e^2, we get

$$(1-e)v= \tfrac{1}{2}F\{x-a(1+e)t\} + \frac{a(1-e)}{2}[1-f\{x-a(1+e)t\}].(16)$$

Also, since

$$\frac{dy}{dx}=\frac{D}{\rho}=\frac{D}{D(1+s)}=1-s,$$

substituting in (15) this value of $\dfrac{dy}{dx}$, we get, multiplying by a,

$$as= \tfrac{1}{2}F\{x-a(1+e)t\} + \frac{a(1-e)}{2}[1-f\{x-a(1+e)t\}];$$

therefore

$$as=(1-e)v. \quad \cdots \cdots \quad (17)$$

Hence, when the motion is represented by the differential equation (12), we shall have a disturbance propagated to the right with

the velocity $a(1 + e)$, which is represented by the equations (16) and (17). The disturbance so propagated will retain the same invariable form; and (17) shows that it will be either a condensation or rarefaction, according as v is positive or negative.

We have hitherto been considering what takes place to the right of the original disturbance when the motion is represented by (12), x being supposed positive when measured to the right.

For points to the left of the original disturbance we shall have

$$x - a(1 + e)t < 0,$$

and therefore

$$F\{x - a(1 + e)t\} = 0, \quad f\{x - a(1 + e)t\} = 1$$

for all values of t.

Hence the motion to the left of the original disturbance will be represented by the equations

$$v = \frac{1 - e}{2} F\{x + a(1 - e)t\} - \frac{a}{2}[1 - f\{x + a(1 - e)t\}],$$

$$\frac{dy}{dx} = \frac{1}{2a} F\{x + a(1 - e)t\} + \frac{1}{2}[1 - e + (1 + e)f\{x + a(1 - e)t\}];$$

and adopting a mode of treatment similar to that employed in the former case, we shall find that when the equation (12) is applicable to the motion, there will be propagated with the velocity $a(1 - e)$ to the left of the original disturbance a disturbance represented by the equations

$$(1 + e)v = \tfrac{1}{2}F\{x + a(1 - e)t\} - \frac{a(1 + e)}{2}[1 - f\{x + a(1 - e)t\}],$$

$$-as = (1 + e)v;$$

from which last it results that the disturbance so propagated will consist of a *rarefaction* when v is positive, and *vice versâ*; which is the opposite of what obtains with regard to the disturbance propagated to the right.

If in (11) we take the lower sign, we get the alternative equation of motion,

$$0 = \frac{d^2y}{dt^2} - 2ae\frac{d^2y}{dx\,dt} - a^2\frac{d^2y}{dx^2}. \quad . \quad . \quad (18)$$

The results which occur when this equation is applicable to the motion will obviously be found by changing the sign of e in the results obtained on the assumption that (12) is the differential equation applicable to the motion.

In this case, therefore, we shall have a disturbance propagated

to the right with the velocity $a(1-e)$ represented by the equations

$$(1+e)v = \tfrac{1}{2}\,\mathrm{F}\,\{x - a(1-e)t\} + \frac{a(1+e)}{2}\,[1 - f\{x - a(1-e)t\}],$$
$$as = (1+e)v\,;$$

while to the left will be propagated, with the velocity $a(1+e)$, a disturbance represented by

$$(1-e)v = \tfrac{1}{2}\,\mathrm{F}\,\{x + a(1+e)t\} - \frac{a(1-e)}{2}\,[1 - f\{x + a(1+e)t\}],$$
$$-as = (1-e)v.$$

It only remains, therefore, to determine under what circumstances (12), and under what circumstances (18) is to be taken as the equation of motion.

Comparing (11) with (1), we get

$$\frac{1}{\mathrm{D}}\frac{dp}{dx} = \pm\,2ae\frac{dv}{dx} - a^2\frac{d}{dx}\cdot\frac{dy}{dx};$$

or, since

$$\frac{dy}{dx} = \frac{\mathrm{D}}{\mathrm{D}(1+s)} = 1-s,$$

$$\frac{dp}{dx} = \pm\,2\mathrm{D}ae\frac{dv}{dx} + \mathrm{D}a^2\frac{ds}{dx},$$

$$\therefore\ p = \pm\,2\mathrm{D}aev + \mathrm{D}a^2s + \text{const.}$$

$$= \pm\,2\mathrm{D}aev + \mathrm{D}a^2(1+s). \quad \ldots \quad (19)$$

Now it is obviously impossible that in any particular case of motion p should have two values. We have therefore to determine in each particular case of motion which of the above values is to be taken.

Suppose that we have at a given epoch, in two different tubes, exactly the same kind of disturbance, with this difference, viz. that the velocity at each point of the one is in the opposite direction to that at the corresponding point of the other. At a point in each for which the values of v and s are identical except as regards the sign of the former, it is clear that the pressure must have the same absolute value; but it is equally clear that *the expression* for the pressure must differ in the two cases.

If in the one case, the particle-motion being to the right, and x being measured positively in the same direction, the pressure is represented by

$$p = +\,2\mathrm{D}aev + \mathrm{D}a^2(1+s),$$

then in the other case, the particle-motion being to the left, x being measured as before, the pressure must be represented by

$$p = -2\mathrm{D}aev + \mathrm{D}a^2(1+s),$$

and *vice versâ.*

The occurrence of the double sign in the value of p and in the (11) is thus at once accounted for. It still remains to be determined, however, whether, when the particle-motion at a given point is to the right, the coefficient of v in the expression for the pressure should have a positive or negative sign affixed to it.

Suppose that, the air being of uniform density and at rest, a disturbance is suddenly impressed upon a limited portion of it of this kind, viz. a velocity which beginning at zero gradually increases till it attains the value v_1, and thence diminishes by the same gradations till it finally vanishes, the density throughout being unaffected. According to the formula (19), we shall have under these circumstances,

$$p = \mathrm{D}a^2 \pm 2\mathrm{D}aev,$$

i. e. the pressure will be either increased or decreased by the impressed velocity.

The sign to be attributed to the coefficient of v in the last expression, equally with the numerical value of the constant e, is a proper subject for experiment; nevertheless I think we may conclude with perfect certainty that the lower sign is to be taken (in other words, that the effect of the impressed velocity is to *diminish* the pressure in the portion of the air affected by it), on the following grounds.

We have before us the following alternative. For the sake of perspicuity, assuming that the particle-motion thus supposed to be impressed tends to the right, we must either have the pressure gradually increasing as we move from the left of the disturbance till we reach its middle point, and thence gradually diminishing until it again assumes at the right-hand extremity the value of equilibrium, or else the pressure will *diminish* from the left-hand extremity up to the middle point, and will thence *increase* till it ultimately regains the value of equilibrium.

But in either case we shall have in the first half of the disturbance (beginning from the left) the particles in each element flying from each other, the tendency, by reason of the impressed velocity, being to expansion; while in the second half the particles in each element are moving towards each other, the tendency in this latter case being towards compression.

It appears, therefore, that we have to choose between two things, viz. on the one hand a diminished pressure where there is throughout a tendency to expansion, and an increased pressure where there is throughout a tendency to condensation; or, on the other hand, an increased pressure where there is throughout a tendency to expansion, and a diminished pressure where there is throughout a tendency to condensation. That this latter alternative should be true appears incredible. We may with safety conclude, therefore, that when the motion is to the right, x being measured positively in that direction, the lower sign is to be taken in (19), and *vice versâ*.

Applying this conclusion to the results previously obtained, it follows that, when the motion is represented by (12), the particle-motion is to the left, and v is negative; so that any disturbance propagated to the right of the original disturbance will be *a rarefaction*, and its velocity of propagation will be $a(1+e)$; while any disturbance propagated to the left must be *a condensation*, whose velocity of propagation will be $a(1-e)$.

On the other hand, when the motion is represented by (18) the particle-motion takes place to the right, i. e. v is positive; so that any disturbance propagated to the right of the original disturbance will in this case be *a condensation,* whose velocity of propagation is $a(1-e)$; while any disturbance propagated to the left must be *a rarefaction,* and its velocity of propagation will be $a(1+e)$.

It results on the whole, therefore, that waves of condensation are propagated with the velocity $a(1-e)$, which is less than what has hitherto been regarded as the calculated velocity apart from temperature; while waves of rarefaction are propagated with a velocity $a(1+e)$, which is just as much greater than such calculated velocity.

If it be asked whether is e so small that the difference between these two velocities is imperceptible to the human ear under all circumstances, or are two perceptibly distinct waves in fact propagated? I answer that e is *not* so small as that the difference between $a(1+e)$ and $a(1-e)$ cannot be distinctly appreciated. Two waves will in fact be propagated, one of which (the slower) the human ear is so constructed as to suppress. The proof of this I reserve for a future communication.

6 New Square, Lincoln's Inn,
    February 16, 1869.

XXVI. *On the Physical Cause of the Motion of Glaciers.*
By JAMES CROLL, *of the Geological Survey of Scotland**.

I HAVE just seen an abstract of a most interesting paper
by the Reverend Canon Moseley " On the Mechanical
Possibility of the Descent of Glaciers by their weight only,"
which was read before the Royal Society on the 7th of January
last †.   In that memoir he arrives at the conclusion that, owing
to the great resistance offered by the solid ice to *shearing*, it is
impossible that glaciers can descend by their weight alone.
   " All the parts," he remarks, " of a glacier do not descend
with a common motion ; it moves faster at its surface than
deeper down, and at the centre of its surface than at its edges.
It does not only come down bodily, but with different motions
of its different parts ; so that if a transverse section were made
through it, the ice would be found to be moving differently at
every point of that section. . . . . . There is a constant displace-
ment of the particles of the ice over one another and alongside
one another, to which is opposed that force of resistance which
is known in mechanics as *shearing-force*."
   He determines by calculation the amount of shearing-force
which must not be exceeded if the displacement of the particles
is to be effected by the weight of the ice alone.   In the case of
the Mer de Glace at the Tacul, the shearing-force of the ice must
not exceed 1·3193 lb. per unit surface of one square inch, if that
glacier descends merely by its weight, at the rate observed by
Professor Tyndall.   From experiments which he has made,
he finds that the actual shearing-force of ice per unit surface is
about 75 lbs.   Consequently he concludes it is impossible that the
motion of the glacier can be due to its weight alone ; there must
be some other force in addition to the weight impelling the ice
forward.   And he calculates that the amount of work performed
by this unknown force is thirty-four times the amount performed
by the weight of the glacier.
   This is a most important conclusion.   It is quite decisive
against the generally received opinions regarding the descent of
glaciers by their own weight.
   But although it is thus demonstrated that glaciers cannot de-
scend by means of their weight alone in the manner generally
supposed, still, I venture to think that, notwithstanding the
demonstration, gravitation after all may be the only force moving
the ice.

   * Communicated by the Author.
   † Proceedings of the Royal Society, vol. xvii. p. 202.   [See p. 229 of
our present Number, ED. *Phil. Mag.*]

The correctness of the above conclusion, that the weight of the ice is not a sufficient cause, depends upon the truth of a certain element taken for granted in the reasoning, viz. that the *shearing-force* of the molecules of the ice remains *constant*. If this force remains constant, then Canon Moseley's conclusion is undoubtedly correct, but not otherwise; for if a molecule should lose its shearing-force, though it were but for a moment, if no obstacle stood in front of the molecule, it would descend in virtue of its weight.

The fact that the shearing-force of a mass of ice is found to be constant does not prove that the same is the case in regard to the individual molecules. If we take a mass of molecules in the aggregate, the shearing-force of the mass taken thus collectively may remain absolutely constant, while at the same time each individual molecule may be suffering repeated momentary losses of shearing-force. This is so obvious as to require no further elucidation. The whole matter, therefore, resolves itself into this one question, as to whether or not the shearing-force of a crystalline molecule of ice remains constant. In the case of ordinary solid bodies we have no reason to conclude that the shearing-force of the molecules ever disappears, but in regard to ice it is very different.

If we analyze the process by which heat is conducted through ice, we shall find that we have reason to believe *that while a molecule of ice is in the act of transmitting the energy received (say from a fire), it loses for the moment its shearing-force if the temperature of the ice be not under* 32° F. If we apply heat to the end of a bar of iron, the molecules at the surface of the end have their temperatures raised. Molecule A at the surface, whose temperature has been raised, instantly commences to transfer to B a portion of the energy received. The tendency of this process is to lower the temperature of A and raise the temperature of B. B then, with its temperature raised, begins to transfer the energy to C. The result here is the same; B tends to fall in temperature, and C to rise. This process goes on from molecule to molecule until the opposite end of the bar is reached. Here in this case the energy or heat applied to the end of the bar is transmitted from molecule to molecule under the form of *heat or temperature*. The energy applied to the bar does *not change its character; it passes right along from molecule to molecule under the form of heat or temperature.* But the nature of the process must be wholly different if the transference takes place through a bar of ice at the temperature of 32°. Suppose we apply the heat of the fire to the end of the bar of ice at 32°, the molecules of the ice cannot possibly have their temperatures raised in the least degree. How, then, can molecule A take on, *under the form*

of heat, the energy received from the fire without being heated or having its *temperature* raised ? The thing is impossible. The energy of the fire must appear in A under a different form from that of heat. The same process of reasoning is equally applicable to B. The molecule B cannot accept of the energy from A under the form of heat; it must receive it under some other form. The same must hold equally true of all the other molecules till we reach the opposite end of the bar of ice. And yet, strange to say, the last molecule transmits in the form of heat its energy to the objects beyond ; for we find that the heat applied to one side of a piece of ice will affect the thermal pile on the opposite side.

The question is susceptible of a clear and definite answer. When heat is applied to a molecule of ice at 32°, the heat applied does not raise the temperature of the molecule, it is consumed in work against the cohesive forces binding the atoms or particles together into the crystalline form. The energy then must exist in the dissolved crystalline molecule, under the statical form of an affinity—crystalline affinity, or whatever else we may call it. That is to say, the energy then exists in the particles as a power or tendency to rush together again into the crystalline form, and the moment they are allowed to do so they give out the energy that was expended upon them in their separation. This energy, when it is thus given out again, assumes the dynamical form of heat ; in other words, the molecule gives out *heat* in the act of freezing. The heat thus given out may be employed to melt the next adjoining molecule. The ice-molecules take on energy from a heated body by melting. That peculiar form of motion or energy called heat disappears in forcing the particles of the crystalline molecule separate, and for the time being exists in the form of a tendency in the separated particles to come together again into the crystalline form.

But it must be observed that although the crystalline molecule, when it is acting as a conductor, takes on energy under this form from the heated body, it only exists in the molecule under such a form during the moment of transmission ; that is to say, the molecule is melted, but only for the moment. When B accepts of the energy from A, the molecule A instantly assumes the crystalline form. B is now melted ; and when C accepts of the energy from B, then B also in turn assumes the solid state. This process goes on from molecule to molecule till the energy is transmitted through to the opposite side and the ice is left in its original solid state. This is the *rationale* of Faraday's property of regelation.

This is no mere theory or hypothesis ; it is a necessary consequence from known facts. We know that ice at 32° cannot take

on energy from a heated body without melting; and we know also equally well that a slab of ice at 32°, notwithstanding this, still, as a mass, retains its solid state while the heat is being transmitted through it. This proves that every molecule resumes its crystalline form the moment after the energy is transferred over to the adjoining molecule.

This point being established, every difficulty regarding the descent of the glacier entirely disappears; for a molecule the moment that it assumes the fluid state is completely freed from shearing-force, and can descend by virtue of its own weight without any impediment. All that the molecule requires is simply room or space to advance in. If the molecule were in absolute contact with the adjoining molecule below, it would not descend unless it could push that molecule before it, which it probably would not be able to do. But the molecule actually has room in which to advance; for in passing from the solid to the liquid state its volume is diminished by about $\frac{1}{10}$, and it consequently can descend. True, when it again assumes the solid form it will regain its former volume; but the question is, will it go back to its old position? If we examine the matter thoroughly we shall find that it cannot. If there were only this one molecule affected by the heat, this molecule would certainly not descend; but all the molecules are similarly affected, although not all at the same moment of time.

Let us observe what takes place, say at the lower end of the glacier. The molecule A at the lower end, say, of the surface, receives heat from the sun's rays; it melts, and in melting not only loses its shearing-force and descends by its own weight, but it contracts also. B immediately above it is now, so far as A is concerned, at liberty to descend, and will do so the moment that it assumes the liquid state. A by this time has become solid and again fixed by shearing-force; but it is not fixed in its old position, but a little below where it was before. If B has not already passed into the fluid state in consequence of heat derived from the sun, the additional supply which it will receive from the solidifying of A will melt it. The moment that B becomes fluid it will descend till it reaches A. B then is solidified a little below its former position. The same process of reasoning is in a similar manner applicable to every molecule of the glacier. Each molecule of the glacier consequently descends step by step as it melts and solidifies, and hence the glacier, considered as a mass, is in a state of constant motion downwards. The fact observed by Professor Tyndall that there are certain planes in the ice along which melting takes place more readily than others will perhaps favour the descent of the glacier.

We have in this theory a satisfactory explanation of the origin

of "crevasses" in glaciers. Take, for example, the transverse crevasses formed at the point where an increase in the inclination of the glacier takes place. Suppose a change of inclination from, say, 4° to 8° in the bed of the glacier. The molecules on the slope of 8° will descend more rapidly than those above on the slope of 4°. A state of tension will therefore be induced at the point where the change of inclination occurs. The ice on the slope of 8° will tend to pull after it the mass of the glacier moving more slowly on the slope above. The pull being continued, the glacier will snap asunder the moment that the cohesion of the ice is overcome. The greater the change of inclination is, the more readily will the rupture of the ice take place. Every species of crevasse can be explained upon the same principle.

This theory explains also why a glacier moves at a greater rate during summer than during winter; for as the supply of heat to the glacier is greater during the former season than during the latter, the molecules will pass oftener into the liquid state.

As regards the denuding power of glaciers, I may observe that, though a glacier descends molecule by molecule, it will grind the rocky bed over which it moves as effectually as it would do did it slide down in a rigid mass in the way generally supposed; for the grinding-effect is produced not by the ice of the glacier, but by the stones, sand, and other materials forced along under it. But if all the resistances opposing the descent of a glacier, internal and external, are overcome by the mere weight of the ice alone, it can be proved that in the case of one descending with a given velocity the amount of work performed in forcing the grinding materials lying under the ice forward must be as great, supposing the motion of the ice to be molecular, in the way I have explained, as it would be supposing the ice descended in the manner generally supposed.

Of course, a glacier could not descend by means of its weight as rapidly in the latter case as in the former; for in fact, as Canon Moseley has shown, it would not in the latter case descend at all; but assuming for the sake of argument the rate of descent in both cases to be the same, the conclusion I have stated would follow. Consequently whatever denuding effects may have been attributed to the glacier, according to the ordinary theory, must be equally attributable to it according to the present theory.

This theory, however, explains, what has always hitherto excited astonishment, viz. why a glacier can descend a slope almost horizontal, or why the ice can move off the face of a continent perfectly level.

Canon Moseley suggests that heat passing into the ice might

by its mechanical energy, together with the weight of the glacier, be sufficient to account for the motion. But the mechanical energy of heat is not required to push the glacier forward; gravitation alone, as we have just seen, will suffice. Besides, heat entering ice could not produce a mechanical pressure that would move the glacier; for heat produces contraction of volume, not expansion. True, heat no doubt destroys the crystalline structure of the ice-molecule by tearing the constituent particles separate; but nevertheless the volume of the mass is diminished by this process, for ice in losing its crystalline structure, or, in other words, in passing from ice to water, decreases in volume.

XXVII. *On* Mr. J. Croll's *paper " On Geological Time, and the probable Date of the Glacial and the Upper Miocene Period"**. *By* R. A. Peacock, *C.E.*†

THE writer hereof believes that Mr. Croll's paper is of great value; and if a few remarks are ventured upon below on small points of detail, they no more detract from the general value of the paper than a few " striæ " or scratches would detract from the value of a good painting.

"The only evidence which we can now reasonably expect to find in the stratified rocks of the existence of land-ice of former epochs, is the presence of erratic blocks which may have been transported by icebergs and dropped into the sea. But unless the glaciers of that epoch reached the sea or the sea was frozen, we could not possibly have even this evidence. Traces in the stratified rocks of the effects of land-ice of former epochs must, from the very nature of things, be rare indeed " (p. 364).

On the contrary, might we not have striation on the stratified rocks pretty often in this way? When we remember the frequent oscillations of land by sinkings and risings in every part of the globe since the commencement of the glacial period (assuming that to have commenced 240,000 years ago and to have lasted 160,000 years), the following may *often* have happened. Suppose (as must have been the case) many glaciers to have been making each its way down its own valley in the usual manner, bearing its lines of moraines, as in Switzerland at present. The glaciers would then striate the stratified rocks of every valley. Such striations would continually go on increasing as long as the glaciers existed. And considering the vast tract of the earth which must have been thus operated on during 160,000

* Philosophical Magazine, November 1868.
† Communicated by the Author.

years, we may be quite sure that by means of sinkings of land
many such striated rocks have become sea-bottom; the conse-
quences of which would be that the glaciers would have melted,
and the stratified rocks would retain their striations, which would
be well protected by the thousands of feet of sea-water they
might have above them. Their moraines of erratic blocks would
be sure to be deposited at the sea-bottom also. And the ground
may have risen again since, and even in some cases risen and
sunk again and again. Denudation would, I fully agree, oblite-
rate all striations which were exposed to it *long enough* in sub-
aërial situations. Again :—

" But do icebergs striate the rocky bed of the ocean ? Are they
adapted for such work ? It seems to be almost universally as-
sumed that they are. But I have been totally unable to find any
rational grounds for such a belief. Clean ice can have but little
or no erosive power, and *never could scratch a rock*" (p. 366).

I must dissent from the words I have italicized. As often
as floating icebergs happen to touch a rocky bottom during
storms, when they are making way through the water like ships,
since they often weigh many thousands of tons, they will still
striate the bottom-rocks, although their own bottom parts will
be ground to powder by the operation. Such bottom parts
would do *something* in the way of striation, as Mr. Croll admits
(p. 367), by the mere force of concussion; for all their force
would not necessarily be expended in tearing up loose and dis-
jointed rocks, nor in hurling loose materials to a distance.

In Jersey there are some remarkable striations or flutings on
the face of the metamorphic clayslate rock, which is nearly per-
pendicular, as you enter the village of l'Etacq from the east, on
the right (*i. e.* the north) side of the road. They were first ex-
posed when the road was widened some eight or ten years ago.
Previously they were covered up with earth, grass, or other
rocks, I do not know which. I have heretofore spoken of these
as "*slickensides*;" but as they are horizontal, it is at least pos-
sible that they may have been striated by a glacier, as they are
at the side of a valley :—
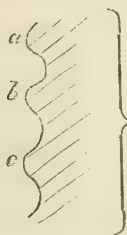
Metamorphic-clay-slate section; the
length 20 or 30 feet.

The flutings *a b, b c,* &c. (of which there are about ten or twelve) are about 4 inches broad and an inch deep.

On page 368 it is stated that " an iceberg 100 feet in thickness will exert just as much pressure as one a mile in thickness." This cannot be so.   An iceberg 100 feet thick when afloat will displace water weighing (suppose) 100 tons, but one a mile thick will displace water weighing (suppose) 10,000 tons. These quantities are equal to the respective weights of the icebergs while afloat.   Now let it be supposed that the two icebergs are each propelled by a storm at the rate of three miles an hour, and it must be evident that, when they each come in contact with a rocky bottom, the latter will exert 100 times as much force as the former and produce striæ far greater.

Jersey, January 25, 1869.

XXVIII. *Contributions to the Knowledge of the Spectra of the Flames of Gases containing Carbon.   By* Andrew Lielegg, *Professor at the National Upper Practical School at St. Pölten, Austria*.*

A T the time when I was making my observations on the spectrum of the Bessemer-flame†, I conceived the idea of investigating what similarity or difference existed between this spectrum (which is, in fact, that of the flame of oxide of carbon) and the spectra of the flames of other gases which contain carbon. It seemed to me that data might also thus be obtained for the solution of the question, whether all spectra of gases containing carbon are really to be regarded as spectra of carbon, or whether every such gas has its own peculiar spectrum.

With this object in view, I undertook to examine the spectra of light carburetted hydrogen, of olefiant gas, and of cyanogen, and came thus to the knowledge, concerning the two former gases, of some details which, in the greater works already before us on this subject, have either not been noticed at all, or at least have not been described as seen in the manner in which I have had the opportunity of observing them.   In communicating, then, in the following pages, these details as contributions to our knowledge of the spectra of ignited bodies, I give also the results which I have been able to obtain from a comparison of the Bessemer-spectrum with the spectra of other flames.

Spectrum of Coal-gas.

When Swan published* his investigations on the prismatic spectra of the flames of compounds of carbon and hydrogen, which are remarkable for their completeness in the then (1855) state of our knowledge in spectrum-analysis, the only known observations were those of Fraunhofer, Brewster, and Draper on the spectrum of the cone of the blowpipe-flame, and those of Fraunhofer, Herschel, and others on the spectrum of a wax- or oil-flame; moreover the means of making this kind of observation had not at that time attained its present high degree of perfection. Under these circumstances, the service rendered by Swan must not be underestimated—in examining the spectra of the flames, not only of carbon combined with hydrogen, but of the various compounds of carbon, hydrogen, and oxygen, in doing this with an accuracy not surpassed even in recent times, and in arriving at conclusions which are still considered true in their fundamental principles, viz. (1) that the position of the bright lines in the spectra of the different compounds of carbon and hydrogen is independent of the relative proportion of those two substances contained in them and is in all cases the same, and (2) that compounds which contain oxygen as well as carbon and hydrogen give spectra identical with those of compounds of the latter two substances only.

Since the time to which I refer, this subject has been again investigated; and particularly Plücker and Hittorf †, as well as H. C. Dibbits‡, have annexed to their publications very excellent drawings; those indeed of the last-named investigator leave nothing to be desired in their repetition. But not one of these authors has mentioned, in treating of the spectrum of coal-gas or olefiant gas obtained by combustion with oxygen, a group of five red lines, which I, in my oft-repeated experiments, have always been able to observe with uniform distinctness, and which decidedly belong to the spectrum of coal-gas.

For the production of this spectrum, I made use of a Daniell's cock of the ordinary construction, with an escape-aperture, narrow in bore compared with that of the tubes by which the coal-gas and oxygen are brought into communication, regulating the quantity of the gases to be combined in such a manner that I obtained a small, nearly globular flame, only narrowing into a very short point at the top, which was of a faint bluish-white colour and an intense brilliancy. A flame thus produced, and brought

* Transactions of the Royal Society of Edinburgh, vol. xxi. part 3, p. 411.

† Phil. Trans. vol. clv. part. 1. p. 15.

‡ De Spectraal-Analyse, Akademische Proefschrift: Rotterdam, E. H. Tassemeyer, 1863.

as near as possible to the slit in the apparatus, exhibited the spectrum of coal-gas with extraordinary sharpness and brilliancy of colour, and furnished the means of observing the five red lines spoken of with great distinctness.

The successive development of the spectrum of coal-gas, which can be easily proved by comparing the spectra which are produced by a Bunsen's gas-burner, then by coal-gas with oxygen in small quantity, and lastly by coal-gas with oxygen in sufficient quantity to create combustion, leads to the conclusion, which has now been known for some length of time, that increase in the temperature of the flame causes a great change in the form of the spectrum ; that is to say, continued augmentation of the supply of oxygen adds more and more lines, whilst at the same time greater intensity of light and brilliancy of colour is shown throughout, and this without any perceptible appearance of change of an opposite character. Beginning with the flame of a Bunsen's gas-burner, and passing on to the hottest gas-flame inflamed by oxygen, a series of spectra may be followed which show no essential difference, since the different degrees of their development cannot be considered such ; for the lines which are produced by Bunsen's gas-burner preserve their position unchanged, other groups being as it were filled in, so that this successive completion of the spectrum can be distinctly followed as the temperature is increased.

It can only be ascribed to this gradual change of the spectrum of coal-gas proceeding from a change of temperature that the group of five lines mentioned above has never before been observed and described as it is in this paper. The flame must be brought, by a sufficient supply of oxygen, to the maximum of its temperature and intensity of light; this group then appears as sharp and distinct as those in the green and blue parts of the spectrum, which become visible at a considerably lower temperature ; in general character also its correspondence with the latter is complete.

In order to obtain a means of reference, in regard to the position and also the breadth of the red lines and those of the spaces between them, to those of the other lines of the spectrum, I made a determination of their relative position by means of an illuminated Steinheil's scale, which divides the space between K α and K β into 255 equal parts. The breadth given to the slit was such that that of the sodium-line just filled up the interval between two divisions ; this breadth is also that of nearly all the lines which form groups. The apparatus thus employed* (the same which I used in my earlier works) has

* From the manufactory of mathematical and scientific instruments, by Starke and Kammerer, at the Imperial Polytechnic Institute of Vienna.

two prisms, which were inserted at the minimum of deviation for D, and a telescope magnifying six times. The following are the results of the readings, with the addition also in the proper places of those of the lines of potassium, sodium, and lithium :—

304 K α.
285 Extreme red.
278 Li α.

$\alpha \begin{cases} 260 \\ 257 \\ 254 \\ 252 \\ 250 \end{cases}$ Group of five red lines.

246 Sodium-line.

$\beta \begin{cases} 233 \\ 230 \\ 227 \\ 225 \\ 222 \end{cases}$ Group of five yellowish-green lines.

$\gamma \begin{cases} 201 \\ 198 \\ 195 \end{cases}$ Group of three pea-green lines, to which also follows a fourth, when the intensity of light of the flame is especially great.

$\delta \begin{cases} 160 \\ 157 \\ 155 \\ 153 \end{cases}$ Group of four bright-blue lines.

$\begin{cases} 112 \\ 107 \end{cases}$ Boundaries of a broad blue band, brightest in the middle, and becoming gradually fainter towards the edges.

103 Narrow bright-violet line.

$\begin{cases} 101 \cdot 5 \\ 98 \end{cases}$ Boundaries of a bright dark-violet band, brightest in the middle (100).

82 Violet end.

It is thus seen that the group of five red lines is situated between the lithium and the sodium line; the three first lines are at equal distances from each other, and are sensibly of equal breadth; the distance of the third line from the fourth, and of the fourth line from the fifth is somewhat less, as is also their breadth, though in a degree less evident to the sight. As regards intensity of light, this group is circumstanced like the others; that is to say, the first or least-refracted line is the brightest, whilst each succeeding line is fainter, the last being faintest of all; this diminution of brightness, however, is not so striking as it is, for example, in the lines of the group γ, and may perhaps even be regarded as merely the effect of contrast.

The pea-green line, 201 of the group γ, and the last violet band, 101·5–98, which (as is well known) nearly coincide with Fraunhofer's lines C and G, are also those lines which are always visible if we use as a source of light a flame of spirit of wine, or the lowest non-luminous blue part of the flame of a wax-light, or the spherical light of the gas-flame of a Bunsen's burner; in the two latter there can also usually be distinctly observed, under favourable circumstances, the second line (198) of the group γ, the first line (233) of the group β, and, lastly, the whole group δ; only the individual lines of the latter are not to be distinguished, the whole group appearing to stand out from the background as a faint broad band.

It is moreover to be remarked that, when the combustion of the coal-gas takes place with an insufficient quantity of oxygen, the group of five red lines cannot be even momentarily discerned, and the red and yellow part of the spectrum generally is not perceptible, the space corresponding to it being quite dark up to the first line of the group β.

Spectrum of Olefiant Gas.

If we burn olefiant gas with oxygen in the same manner as we have described for coal-gas, we obtain a spectrum which agrees perfectly with that of coal-gas, and presents only in the development of the extreme violet part (appropriately called by Brücke* lavender-grey) a form quite peculiar, in that it is intersected by a great number of strong black lines, which are arranged near each other in the dark violet part fine and narrow, but in this lavender-grey part, immediately following it, become increasingly broader, and also are separated by longer intervals, until, again approaching nearer, they at last terminate in a broad dark streak, succeeded by a lavender-grey one of equal breadth, at which the visible part of the spectrum ends.

The extent and appearance of this part of the spectrum, which comes immediately after that of the coal-gas, will be at once understood by an inspection of the following results of the measures made of it :—

95 ⎱ Blue-violet space intersected by fine black lines, the light
71 ⎰   constantly decreasing in intensity towards 71.

70 ⎱ Dark space, black at 61.
51 ⎰

    ⎧ Lavender-grey intersected by many black lines, which
50 ⎪   continually increase in breadth, as well as the inter-
7 ⎨   spaces, and in the last third of the whole space again
    ⎩ become gradually narrower, but in a less degree †.

* Poggendorff's *Annalen*, vol. lxxiv. p. 461.
† The violet potassium line K β has the position 49.

5 Middle of a broad black line.
2 Middle of a lavender-grey band*.
1 End.

This peculiarity of the spectrum, which imparts to its most refrangible part a character of quite an opposite kind to that of the remaining parts, could never be distinctly observed in the spectrum of coal-gas, although in the latter also fine black lines are perceptible in the dark-violet end.   In other respects this spectrum agrees perfectly (as has been already stated) with that of coal-gas ; for the appearance of a fourth line in the group γ, as well as of a group of four faint lines between γ and δ, to which the values 186, 183, 181, and 170 of our scale correspond, in no way affects the distinctive type of this spectrum, and is only seen under circumstances especially favourable to the observation.

The agreement which is indubitably apparent on a comparison of the spectra which are obtained by a flame of spirit of wine, by a flame of a wax-light in the blue part, by coal-gas ignited with atmospheric air or oxygen, and by olefiant gas with oxygen, justifies the assertion that it is entirely the greater or smaller quantity of the luminous particles combined with the higher or lower temperature that produces the gradually seen difference in these spectra, and that all must be in themselves of the very same quality.   Whether the luminous particles of disintegrated carbon are in the condition of vapour, as has been repeatedly assumed, and whether the opinion first expressed by Attfield †, that the spectra of all combinations of carbon are to be considered as spectra of carbon itself, will be confirmed, or whether every gas containing carbon has its own peculiar spectrum,— these are questions which, in regard to the spectra of flames, the present state of our knowledge does not enable us to decide. If, however, we are careful to use only facts in our discussions of the problem, and remember that the presence of carbon in a gaseous condition is only an hypothesis, and even those cases in which this hypothesis appears justifiable, as in Geissler's tubes, manifest relations of so different a nature that reasoning from them is an inadmissible process—and if, moreover, we take into consideration the great difference shown by the spectra of cyanogen and carbonic oxide compared with those of coal-gas and other kindred substances, we shall not be able to adopt the view that all spectra of flames of compounds of carbon can be regarded as spectra of carbon itself.   In addition to this, the in-

* The bright lines of the most refracted group of the cyanogen-spectrum, which also are of a lavender-grey colour, correspond precisely with this position.
† Edinburgh Phil. Trans. vol. xxii. p. 224.

teresting investigations also of Frankland *, according to which the intensity of light of a flame depends upon the density of the ignited vapours, cannot but lead us to hesitate in supposing carbon to exist in a state of vapour in such flames; so that it appears simpler to refer the flame-spectrum of the compounds of carbon with hydrogen, and of oxygen with carbon and hydrogen, to the ignited vapours of carbon combined with hydrogen than to those of carbon alone.

Since, then, it would seem that no independent spectrum of carbon exists, and that, instead of this, every gas containing carbon has its own peculiar spectrum, and since the quantitative proportions of any compound have no effect upon the character of the spectrum, we have a sufficient explanation why it is that the flames of spirits of wine, wax-lights †, coal-gas, and olefiant gas exhibit the same spectrum, whilst, on the contrary, cyanogen and carbonic oxide, in which bodies of a different nature become the sources of light, manifest an essential difference in their spectra.

In support of this view, similar cases may also be adduced in combinations which have a different nature in their chemical constitution—for instance, the spectra of the compounds of copper with chlorine, bromine, and iodine, observed by A. Mitscherlich ‡ and, on account of their characteristic differences, proposed by him to be applied to analytical purposes. There is no question that these spectra are due, not to the copper, but to the combinations in question, and that the difference shown by them is produced by the substance united with the copper, as in the combinations of carbon it is produced by hydrogen, oxygen, and nitrogen.

Spectrum of Carbonic Oxide.

When combustion of carbonic oxide with atmospheric air or with oxygen is produced, a continuous spectrum is obtained without bright or dark lines, in which the green and blue parts are especially well developed. A flame, however, of carbonic oxide, caused by the combustion of charcoal in a blast-furnace, in which carbonic oxide is consumed at a tolerably high temperature, shows in the continuous spectrum some bright lines; the higher the temperature of the carbonic oxide the more lines appear; and it is evident that the conditions, under which the formation and combustion of the carbonic oxide takes place during a " charge " in the Bessemer-process, are particularly favourable for the production of a spectrum with complete lines.

* Polytechnic Journal, vol. clxxxv. p. 279.

† The lowest blue part of the flame of a wax-light shows, if a cold body be held in it, a white border.

‡ Fresenius, *Zeitschrift für analytische Chemie*, 1865, p. 153. [Phil. Mag. Dec. 1865, p. 449.]

Having already described this in my previous papers on this subject, I will only add here those remarks which are suggested by a comparison with other spectra of gases containing carbon ; they may be summed up in the following points :—

1. The lines of a flame of carbonic oxide (Bessemer-flame) appear on a continuous spectrum, and contain several groups of bright lines and some dark absorption-streaks, which are irregularly distributed from the red up to the violet end.

2. The groups of lines coincide neither with those of coal-gas and olefiant gas, nor with those of cyanogen.

3. The strongest lines are situated, as in the spectrum of coal-gas, in the green and blue-violet part.

4. The increase in the intensity of light of the individual lines of the groups, when such an increase is perceptible at all, takes place always in the same direction : but this is the opposite direction to that observed in all other spectra of gases containing carbon ; for the most refrangible line of each group is the brightest, and those succeeding it invariably become fainter.

5. The spectrum shown by a Geissler's tube filled with carbonic oxide is not the same as that of the flame of carbonic oxide, since both the position and the distribution of the bands and lines are different.

The spectrum, then, of a flame of carbonic oxide is such that it must unquestionably be considered as one peculiar to itself—the spectrum of ignited carbonic oxide.

Spectra of the flames of compounds of carbon and hydrogen, of cyanogen and of carbonic oxide, never show the lines of hydrogen relatively to those of nitrogen and oxygen ; but from this we can only infer either that the lines of these last-named gases do not appear on any ground of such a kind (as is the case in the spectrum of chloride of potassium, in which also the potassium lines alone are visible), or that the molecules of the three first-named kinds of gas as such form the luminous matter when in the state of most intense ignition.   Now, as the spectra are of different characters, they cannot be referred to a common origin, namely, to the carbon alone ; and from this we are entitled to argue that every gas has its own peculiar spectrum, in so far as it possesses a difference of quality in regard to its constitution.

Lastly, as to the comparison of the spectra of the flames of gases with the spectra given by them when in the condition of greatest rarefaction and made luminous by the induced electric current, I believe I ought not to conceal my opinion that it is really inadmissible ; if the electric current is able to decompose so many bodies in their natural condition of density, how much more must it be able to do so when that density is so

greatly diminished ! the mass, being so very small in proportion, will follow completely the impulse of the motion produced by the electric current, and be decomposed in quick succession into its constituents, which are afterwards again united. Therefore tubes filled with combinations of carbon and hydrogen show the lines of carbon and those of hydrogen ; tubes filled with carbonic oxide or carbonic acid gas show those of carbon and oxygen, giving, in fact, a spectrum of carbon, because the extremely small pressure and the high temperature cooperate in reducing the carbon to a gaseous condition. Tubes filled with cyanogen are not adapted for the observation, because the capillary tubes become immediately blackened by decomposed carbon, by which means observation is prevented.

[Professor Lielegg's paper is accompanied by a Plate containing diagrams of the solar spectrum with the more important of Fraunhofer's lines, the spectrum of olefiant gas and of coal-gas (which are identical, except that the latter terminates at 82), and the spectrum of the Bessemer-flame. These, however, are not essential to the paper, and are here omitted.—W. T. L.].

XXIX. *The Secular Effects of Tidal Action.* By DANIEL VAUGHAN, *Esq.*[*]

I N tracing the mutual relations between the physical forces, it is important to show that the occurrence of tides, while attended with friction and with a consequent development of heat, must involve some permanent alteration in the momentum of the vast orbs which are concerned, either in exciting or in restraining the great movement of the liquid domain. An estimate of the amount of the minute changes which this cause may slowly occasion in planetary motion is also intimately connected with some of the great problems of practical astronomy. The earth and the moon have been found to differ slightly in recording time since the earliest astronomical observations ; and much of this difference is now generally ascribed to a gradual reduction in the diurnal velocity of our planet as its watery envelope is alternately elevated and depressed by lunar attraction. The vast tidal force which some of the satellites of Jupiter and Saturn must feel, in consequence of the powerful attraction of the primaries, would be sufficient to cause perpetual oscillations even in the solid matter of these subordinate worlds, if they were not secured by some special means against such desolating effects from the great disturbance. But it is reasonable to suppose that the motion of the satellite would change in proportion to

* Communicated by the Author.

the amount of heat generated by these oscillatory movements; and more exact investigations show that the changes which take place under such circumstances would ultimately cause these second-rate planets to occupy the same time in their rotation and in their orbital revolution, while the axis of each would become nearly, if not exactly, perpendicular to the plane of its orbit. On the surfaces of bodies having their motions so ad- justed in the course of time, gravity, though not uniform in dif- ferent localities, would be exempted from any material periodical changes, and the repose of the planetary structure could not be seriously affected by the central disturbance. From the evidence of the telescope, it would appear that this peculiar arrangement for keeping the same side of a moon always turned to its primary invariably prevails in all secondary systems of celestial bodies; and it may justly claim much interest from astronomers, whether it be regarded as a beneficent provision for preventing excessive tides, or as the result of tidal action during past ages.

Though the great disturbance to which satellites are exposed in narrow orbits, and which, in the absence of certain conditions, is capable of producing commotions even in the most stubborn solid matter of which they may be composed, has been the main object of my researches in this department of science, I think it advisable to introduce a few items in regard to the tides on our own globe. In a paper published in the twenty-seventh volume of the Reports of the British Association for the Advancement of Science, I gave a popular exposition of the mode in which the vast tidal wave on our oceans must permanently alter the mo- tions of the earth and the moon. I have since learned that the views I then presented respecting the loss of terrestrial motion were not entirely new; and as I understand that the subject is already receiving much attention from some eminent astrono- mers, I do not think it proper to treat on it at a greater length than the main object of my researches demand. I shall accord- ingly take up only the more simple cases of this kind of action, as they will be sufficient to show how the secular changes with which the tides on our oceans must be attended are divided be- tween our earth and its satellite.

For the source of the slow changes in question, we may first look to the effects of the attraction between the moon and the portion of water which swells, by her influence, above the mean level of the ocean. The presence of so large an amount of pro- tuberant matter on opposite sides of the terrestrial spheroid must produce a slight deviation in the direction of gravity between the earth and the moon, and thus lead to permanent changes in their movements. To determine the relative rates at which motion is slowly lost to each body from this cause, and to show how far one

can be depended upon for correcting the time kept by the other, is not difficult if the moon's orbit is supposed to be an exact circle. The centrifugal forces arising from the movement of the earth and the moon around the common centre of gravity between them, must have a resultant in a line passing through the centre of gravity of our planet. In the absence of tides, the resultant of the attraction between the two great bodies would, in its mean position, meet the earth's axis which passes through the same centre ; for though it may be caused to deviate from this position by the influence of mountains or by irregularities in the density of terrestrial strata, the deviation would take place to an equal extent on opposite sides of the axis, and would have its effect evenly compensated in the course of every period of rotation. But the influence of the swollen tidal waters causes the attractive force of the earth on the moon to act in a line passing a little east of the terrestrial axis ; and it is on this slight deviation from the axis that the permanent change of motion in both orbs depends. Let D denote the distance between the centres of the earth and the moon, R the measure of the attraction between both bodies, and l the shortest distance between the earth's axis and the line which marks the direction of this attractive force ; then R may be resolved into three components—one coinciding in direction with centrifugal force which it balances, a second depending on the ellipticity of our planet, and serving to produce the precession of the equinoxes and the nutation of the earth's axis, while the third, much smaller in magnitude, depends on the attraction of the tidal waters on the moon. If the last component (which acts in the direction of lunar motion) be represented by f, then from the principles of the resolution of forces it will appear that

$$f = \frac{Rl}{D}. \qquad . \qquad . \qquad . \qquad . \qquad . \qquad . \qquad . \qquad (1)$$

From the theory of rotation it may be also easily inferred that if f' denote the force exerted in changing the earth's rotation by the action of R, then

$$f' = \frac{Rl \cos s}{r}, \qquad . \qquad . \qquad . \qquad . \qquad . \qquad (2)$$

where r stands for the earth's equatorial radius, and s for the moon's declination.

From these expressions it follows that f' is equal to $f \dfrac{D \cos s}{r}$; and the changes of momentum resulting from f and f' in the same time, being proportional to the forces themselves, must have one to the other the ratio of r to D cos s. In this we may observe a conformity to the principle of the preservation of areas and moments. Laplace has shown that a similar relation sub-

sists between the changes which the spheroidal form of the earth occasions in lunar and terrestrial motion.

It is easy to estimate with more accuracy than is common to this department of science the ratio between the minute errors of the earth and the moon in recording mean time, and thus to derive important information respecting the portion of these errors discoverable by observation. As the moon, while only $\frac{1}{80}$ of the earth in respect to mass, moves in its orbit with about $2\frac{1}{6}$ times the diurnal velocity of our equator, our planet, supposing it to be homogeneous, would derive from its rotation about fifteen times the momentum which the moon owes to her orbital motion. Now, the action to which each body is exposed from tidal movements being in the ratio of sixty to one, our globe must lose four infinitesimal parts of its angular velocity from the disturbance while the moon gained one, the mean distance between both orbs being regarded as immutable. But it is well known that if the moon's velocity were increased 1 per cent., there would be an augmentation of 2 per cent. in the size of her orbit, and of 3 per cent. in the time of her periodical revolution. From this it follows that the relative change in the mean motion of our satellite, from the occurrence of the tides, is about three-fourths of that which the length of the day must experience from the same cause, and that only one-fourth of the permanent change thus occasioned in the rotation of our planet would be revealed by a comparison of ancient and modern eclipses.

This result, however, requires some corrections, not only for the inequalities of density in terrestrial matter, but also for the ever varying declination of the moon. To correct for declination, put L for the moon's longitude, I for the inclination of its orbit to the plane of the equator, M for the mass of the earth, and K for its radius of gyration, and $\delta\omega$ for the amount of the secular change in its angular motion in a given time. Then in equation (2) substituting $\dfrac{M k^2 d\delta\omega}{dt}$ for f' in conformity with the principles of dynamics, and for cos s its equal $\sqrt{1-\sin^2 L \sin^2 I}$, extracting the square root of the latter in a series and reducing, we obtain

$$\frac{M K^2 d\delta\omega}{dt} = \frac{R l}{D}\left(1 - \frac{\sin^2 I}{4} + \frac{\sin^2 I \cos 2L}{4} \&c.\right). \quad (3)$$

Now if L be expressed in terms of the time t, and I for the present be regarded as constant, the terms containing L will disappear on integration, and the last equation will yield the following:

$$M K^2 \delta\omega = \frac{R l}{D}\left(1 - \frac{\sin^2 I}{4}\right) \text{ nearly.} \quad (4)$$

If the moon moved in the plane of the ecliptic, this formula

would indicate a reduction of nearly 4 per cent. in the estimate deduced for the earth's loss of diurnal motion, on the supposition that the lunar orbit coincides with the plane of the equator. Had we taken the actual case, in which the moon's path has a small inclination to the plane of the ecliptic, I should be regarded as variable, and the investigation would be more complicated; yet it may be easily seen that the result would not materially differ from that given in formula (4).

While the inclination of the moon's orbit to the equator reduces the change in the earth's diurnal motion 4 per cent., an opposite effect arises from the increasing density of the matter towards the central region of our world. The law to which the variable density of terrestrial strata conforms is not at present sufficiently well defined to serve as a basis for an accurate mathematical investigation; but judging from the effects of this cause on the form of the earth and the precession of the equinoxes, we may safely state that it cannot add more than 12 per cent. to the secular loss which the tides occasion to the motion of our globe; and accordingly a little less than one-third of this loss would be made known by a comparison of the observations on the moon in ancient and modern times. How far these results are liable to be vitiated from the action of a resisting medium in the celestial space, or from the slow contraction of our globe as it constantly parts with internal heat, is a question which cannot be embraced in the present inquiry.

To obtain from the preceding equations a numerical estimate of the extent to which lunar and terrestrial motions are affected by tidal action, it is necessary that the value of l should be known. The most reliable data for determining this value might be derived by observing the small amount of deviation which the position of the moon or the swelling of the tides occasions in the direction of plumblines extending from the top to the bottom of deep mines in various localities. But for obtaining such an estimate from the results of observations already made on the tides a different mode of investigation must be adopted. This I shall now present in the most simple form, my object being not so much to determine how much the length of the day or the path of the moon has changed during long periods of time, as to afford the means for showing the manner in which the power which the tides supply comes indirectly from the immense stores of force which our globe and its satellite derive from their great velocities.

I shall take first the least complicated case, in which the terrestrial waters are supposed to be confined to a uniform channel encircling the earth at the equator, the plane of which I shall regard as coincident with that of the lunar orbit. Let M and m

represent the masses of the earth and the moon, r the equatorial radius of the former, D the distance between the centres of both bodies, A the difference of terrestrial longitude between the crest of the tide-wave and that place where the moon is vertical, ϕ the difference of longitude between the crest of the wave and any point in the channel; and let y be the variable height of the tide at this point; while the breadth of the channel is denoted by b, and is supposed to be small in comparison to the earth's dimensions. Then $brd\phi$ will express the magnitude of the infinitesimal portion of the water which rises above the proper geometrical boundary of the terrestrial spheroid, and its attractive force on the moon will be

$$\frac{k^2 g b y r d\phi}{\mathrm{D}^2 - 2\mathrm{D}r\cos(\mathrm{A}-\phi) + r^2\cos^2(\mathrm{A}-\phi)}, \quad \cdot \quad \cdot \quad (5)$$

in which g measures the attraction exerted at the distance k by a volume of water assumed as the unit of matter and having a spherical form. Of the force represented by formula (5), the component acting horizontally and tending to change the direction of terrestrial gravity on the moon will be

$$\frac{k^2 b g y r^2 \sin(\mathrm{A}-\phi)\,d\phi}{\left(\mathrm{D}^2 - 2\mathrm{D}r\cos(\mathrm{A}-\phi) + r^2\cos^2(\mathrm{A}-\phi)\right)^{\frac{3}{2}}}. \quad \cdot \quad (6)$$

Making this expression equal to df, transforming it into a series of which it is necessary to retain only the first two terms, and putting C for $k^2 gb$, there results

$$df = \frac{\mathrm{C}yr^2\sin(\mathrm{A}-\phi)d\phi}{\mathrm{D}^3} + \frac{3\mathrm{C}yr^3\sin 2(\mathrm{A}-\phi)d\phi}{2\mathrm{D}^4}. \quad \cdot \quad (7)$$

The value of the force f which occasions the secular change in the lunar movement may be obtained by integrating the last equation; but for this purpose y must be expressed in terms of ϕ; and in ascordance with the theory of the tides and the laws of periodicity in their movements, y may be assumed equal to $h\cos 2\phi + h'\sin 2\phi$; h and h' being two constant quantities, the latter small in comparison to the first, and depending on the effect of friction. Equation (7) becomes, on the substitution of this value of y,

$$df = \begin{aligned}
&\frac{\mathrm{C}hr^2\sin(\mathrm{A}+\phi)d\phi}{2\mathrm{D}^3} - \frac{\mathrm{C}hr^2\sin(3\phi-\mathrm{A})d\phi}{2\mathrm{D}^3} \\
&+ \frac{3\mathrm{C}hr^3\sin 2\mathrm{A}d\phi}{4\mathrm{D}^4} - \frac{3\mathrm{C}hr^3\sin(4\phi-2\mathrm{A})d\phi}{4\mathrm{D}^4} \\
&+ \frac{\mathrm{C}h'r^2\cos(\mathrm{A}+\phi)d\phi}{2\mathrm{D}^3} - \frac{\mathrm{C}h'r^2\cos(3\phi-\mathrm{A})d\phi}{2\mathrm{D}^3} \\
&+ \frac{3\mathrm{C}h'r^3\cos 2\mathrm{A}d\phi}{4\mathrm{D}^4} - \frac{3\mathrm{C}h'r^2\cos(4\phi-2\mathrm{A})d\phi}{4\mathrm{D}^4}
\end{aligned} \right\} \quad \cdot \quad (8)$$

Now, on integration within the limits of $\phi = 0$ and $\phi = 2\pi$, the terms containing this variable angle disappear; and since A is constant, there results

$$f = \frac{3\mathrm{C}\pi r^3}{2\mathrm{D}^4}\ (h \sin 2\mathrm{A} + h' \cos 2\mathrm{A}). \quad . \quad . \quad . \quad (9)$$

If the waters of our globe, instead of being confined to the vicinity of the equator, were made to occupy a number of regular channels ranging with the parallels of latitude, and if br denote the breadth of one of these channels, O the polar distance of its middle part, while the notation already given is retained for the remaining items, the tidal swelling of the fluid confined under the given parallel will have its tangential action on the moon expressed by

$$\frac{3\pi k^2 g b r^3 \sin^3 \mathrm{O}}{2\mathrm{D}^4}\ (h \sin 2\mathrm{A} + h' \cos 2\mathrm{A}). \quad . \quad (10)$$

This may be easily found by using in the foregoing investigation $r \sin \mathrm{O}$, the radius of the parallel of latitude, instead of r the radius at the equator. Now instead of the complicated case which our terraqueous world presents, I shall, like most writers on the tides, take an equivalent one in which the entire globe is supposed to be covered with water having a depth equal in all places, or varying regularly with the latitude according to some obvious law. We may suppose this hypothetical ocean divided into watery zones or canals by partitions parallel to the equator; as the number of these divisions become infinite, the breadth of each will be represented by $r d\mathrm{O}$, and its tangential force on the moon by $d\mathrm{F}$. From formula (10) there is thus obtained

$$d\mathrm{F} = \frac{3\pi k^2 g r^4 \sin^3 \mathrm{O}\, d\mathrm{O}}{2\mathrm{D}^4}\ (h \sin 2\mathrm{A} + h' \cos 2\mathrm{A}). \quad (11)$$

The angle A may without much error be regarded as constant for all latitudes; but it is proper to consider the greatest height of the tide-wave as depending on the distance from the equator; and supposing it proportional to the cosine of the latitude, we must substitute for h and h' in the last equation $h \sin \mathrm{O}$ and $h' \sin \mathrm{O}$. Making this substitution and integrating within the limits of $\mathrm{O} = 0$ and $\mathrm{O} = 90°$, we obtain

$$\mathrm{F} = \frac{9k^2 g \pi^2 r^4}{32\mathrm{D}^4}\ (h \sin 2\mathrm{A} + h' \cos 2\mathrm{A}). \quad . \quad . \quad (12)$$

Some idea of the small effect of the tides on lunar motion may be derived from a numerical estimate of the value of F in the last equation, supposing the angle A to be $45°$, and the greatest height of the tides equal to three feet. For this purpose it will be most convenient to take for the unit of attracting matter a

sphere equal to the earth in size and having the density of water; k would then be equal to r; and according to the most recent estimates of the earth's density g would indicate a velocity of five feet a second generated in a second of time. Now $\dfrac{r^4}{D^4}$ is nearly equal to $\dfrac{1}{129060000}$, and the product $\pi^2 k^2 h \sin 2A$ will represent a quantity of matter which, with the unit of measure I have assumed, will be $\dfrac{1}{3000000}$. The velocity due to the force F in a second of time will be expressed by the following fraction of a foot per second:

$$\frac{1}{2764800000000000}.$$

This insignificant force acting on the moon for three millions of years would change her velocity a little more than 1 per cent.; and through the indirect influence on her orbit an increase of about 3 per cent. would be then occasioned in her period of revolution around our planet. If in this estimate I have assigned too low a value for the height of the equatorial tides, there is an ample compensation for the error by giving to the angle A the value necessary for producing a maximum effect.

The relation already exhibited between the change in lunar and terrestrial motion may be also deduced by investigating the loss in the earth's rotation from the reciprocal attraction of the moon on the protuberant tidal waters in a channel either coincident with the equator or parallel to it. To arrive at an approximate estimate of this loss, in the first case the tangential force proceeding from lunar attraction must, with the notation already used, be expressed by $\dfrac{3k^2 gmr \sin 2(A-\phi)}{2D^3}$; and the momentary decrease in the momentum of terrestrial matter from the action of this force on the small portion of the fluid represented by $bryd\phi$ will be

$$\frac{3k^2 gbyr^2 m \sin 2(A-\phi)d\phi\, dt}{2D^3}. \qquad \cdots \quad (13)$$

On making the substitutions already employed for y and $k^2 gh$, the formula becomes

$$\left.\begin{aligned}&\frac{3Chmr^2 \cos 2\phi \sin 2(A-\phi)d\phi\, dt}{2D^3}\\[1ex]&+\frac{3Ch'\, mr^2 \sin 2\phi \sin 2(A-\phi)d\phi\, dt}{2D^3}\end{aligned}\right\} \quad \cdots \quad (14)$$

Reducing and integrating with reference to $d\phi$, taking the limits of $\phi=0$ and $\phi=2\pi$, there results

$$\frac{3\pi Chmr^2 \sin 2A dt}{2D^3} + \frac{3\pi Ch'mr^2 \cos 2A dt}{2D^3}. \quad \cdots \quad (15)$$

This is the loss of terrestrial momentum in the instant of time dt; and the loss must fall on the water alone if its movements were wholly unimpeded by friction. It thus appears that the waste of motive force is sixty times as great to the earth as to the moon, as may be seen by comparing the last formula with equation (9) after multiplying the latter by m. The same conformity to the law for the preservation of areas may be shown for zones of water parallel to the equator in every latitude.

While the enlargement of the moon's orbit through tidal influence converts her apparent gain into an actual loss of velocity, a corresponding result of indirect action would be exhibited in our liquid domain if no friction retarded its movements. Were the terrestrial waters confined to regular channels ranging with the equator or the parallels of latitude, the constant loss of motion would serve to increase the gravity and the pressure of the fluid. But if an ocean of uniform depth covered the entire earth, and if its bottom were perfectly smooth, its waters, though losing some velocity by tidal movements, would have their velocity of rotation increased by retiring towards the polar regions as the centrifugal force declined. In the aqueous envelope of the earth there would accordingly be a gain of momentum, while a loss occurred to the moon on a corresponding scale and from the same cause. But the result is much modified by friction, which makes the oceanic waters partake of the velocity of our planet, and occasions a consumption of motion proportional to the calorific energy of the tides.

The effects of the impediments to the great movement of our seas may be readily understood from what is known to attend the collision of imperfectly elastic bodies. If a large meteorite moving from west to east directly over the equator, and having a circular orbit coincident with the verge of our atmosphere, were to have its planetary career arrested by striking a very high mountain, the collision would occasion no loss of momentum; for whatever the body parted with must be gained by the earth; but the sum of the living forces which the earth and the meteorite possess, and which are measured by the masses multiplied by the square of the velocities, would be diminished in proportion to the amount of heat developed as the meteorite struck the mountain or incorporated with our planet in any other way. There is a similar destruction of living force and a corresponding development of heat from the rolling of the vast bodies of water over the asperities in the bed of the ocean; and motion is ever annihilated in giving birth to calorific energy. Yet nothwithstanding the effects of friction, much of the velocity which the moon gives the liquid domain is retained for some time and exhibited in the production of oceanic currents; but as the force

of these currents is called into requisition for the works of nature or of art, and the water is made to partake of the velocity of the bed on which it rests, the store of force in our planet must be wasted and the length of our day augmented.

A more definite relation between the destruction of force in this manner and the consequent change in planetary motion may be shown by investigating the extent to which a satellite revolving close to its primary has its orbit altered by tidal action arising from the eccentricity of the ellipse which it describes, supposing the rotation adjusted for keeping the same side always turned to the central body.   To this problem other solutions may be given besides that which I presented in the Philosophical Magazine for December 1851.   To seek for evidence of the correlation of forces by physical inquiries of cases hitherto untried or relating to phenomena presented in distant systems is as legitimate as the course pursued by Newton and his followers, who applied all the vast resources of mathematics, not for calculating the course of projectiles near the earth's surface, but for determining the orbits which solar attraction would give bodies moving with immense velocities through the distant realms of space.

Cincinnati, January 21, 1869.

[To be continued.]

XXX. *The Story of an Equation in Differences of the Second Order.   By* J. J. SYLVESTER[*].

MY recent researches into the order of the various systems of equations which serve to determine the forms of reducible cyclodes have led me to notice an equation in the second order of differences which I imagine is new, and possesses a peculiarly interesting complete integral.

If we call

$$fx = (x^2 - a^2)^i (x^2 - b^2)^j (x^2 - c^2)^k \ldots,$$

and $(i, j, k, \ldots$ &c. being given) determine $a, b, c, \ldots$ &c. so as to make $(fx)^2 + (f'x)^2$ a complete square, and if we suppose the indices $i, j, k, \ldots$ to consist of λ integers of one value, μ integers of a second value, ν of a third, and so on, the number of solutions of the problem will *in general* depend not on i, j, k, but on the derived integers $\lambda, \mu, \nu, \ldots$; and we may denote the maximum value of this number by the type $[\lambda, \mu, \nu, \ldots]$[†].

[*] Communicated by the Author.

[†] *Ex. gr.* if

$$fx = (x^2 - a^2)^i (x^2 - b^2)^j (x^2 - c^2)^k (x^2 - d^2)^l,$$

the type is [1, 1, 1, 1], of which the maximum value is 9; but if the sum

Now I have been able to establish the following theorem of derivation as a particular case of a more general one of which the clue is in my hands :—

$$[1, \lambda, \mu, \nu, \ldots] = [\lambda, \mu, \nu, \ldots] + \Sigma(\lambda^2 - \lambda)[1, \lambda - 2, \mu, \nu, \ldots]$$
$$+ 2\Sigma\lambda\mu[1, \lambda - 1, \mu - 1, \nu, \ldots].$$

Suppose now that $\lambda, \mu, \nu, \ldots$ all become unity, and that we call

$$[1, 1, 1, \ldots \text{ to } n \text{ terms}] = \Omega_n,$$

then the theorem above stated gives the relation

$$\Omega_n = \Omega_{n-1} + (n-1)(n-2)\Omega_{n-2}.$$

But by virtue of the form of the equations for finding fx, I know independently that Ω_n is the product of n terms of the progression

$$1, 1, 2, 2, 3, 3, 4, \ldots$$

Hence we have one particular solution of the above equation in differences. To find the second, if we make Ω_1 and Ω_2, 1 and 2 respectively instead of 1, 1, it will be found that the nth term becomes the product of n terms of the analogous progression $1, 2, 2, 4, 4, 6, 6 \ldots$. Thus, then, we are in possession of the complete integral of the equation

$$u_{x+1} = u_x + (x^2 - x)u_{x-1},$$

viz.

$$u_{2x} = \text{C} . 1^2 . 3^2 . 5^2 \ldots (2x-1)^2 + \text{K}2^2 . 4^2 \ldots (2x-2)^2 2x,$$
$$u_{2x+1} = \text{C} . 1^2 . 3^2 . 5 \ldots (2x-1)^2(2x+1) + \text{K} . 2^2 . 4^2 \ldots (2x)^2.$$

Writing $u_x = 1 . 2 . 3 \ldots (x-1)v_x$, the above equation takes the remarkably simple form

$$v_{x+1} - v_{x-1} = \frac{v_x}{x} *.$$

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of any two of the quantities i, j, k, l happens to become equal to the sum of the other two, the order sinks and is either 8 or 7 ; I am not quite certain which at present, although it is more probably the former.

* Whether taken under this or the original form, the equation will be found to lie *outside* the cases of integrable linear difference of equations of the second order with linear or quadratic coefficients given by the late Mr. Boole in his valuable treatise on finite differences. In the second form the solution ought by Laplace's method to be representable by a definite integral. Expressed under the more ordinary form the integral is as follows :

$$v_{2x} = \text{C}\frac{3 . 5 . 7 \ldots (2x-1)}{2 . 4 . 6 \ldots (2x-2)} + k\frac{2 . 4 . 6 \ldots (2x)}{1 . 3 . 5 \ldots (2x-1)},$$
$$v_{2x-1} = \text{C}\frac{3 . 5 . 7 \ldots (2x-1)}{2 . 4 . 6 \ldots (2x-2)} + k\frac{2 . 4 . 6 \ldots (2x-2)}{1 . 3 . 5 \ldots (2x-3)}.$$

The romance of algebra presents few episodes more wonderful than this specimen of the way in which the determination of the degree of an equation resulting from elimination can be made to contribute a new and by no means obvious fact to the Calculus of Differences.

Athenæum Club,
February 23, 1869.

XXXI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 145.]

December 17, 1868.—Captain Richards, R.N., Vice-President, in the Chair.

THE following communication was read :—
"On the Measurement of the Luminous Intensity of Light." By William Crookes, F.R.S. &c.

The measurement of the luminous intensity of a ray of light is a problem the solution of which has been repeatedly attempted, but with less satisfactory results than the endeavours to measure the other radiant forces. The problem is susceptible of two divisions, the absolute and the relative measurement of light.

A relative photometer is one in which the observer has only to ascertain the relative illuminating powers of two sources of light, one of which is kept as uniform as possible, the other being the light whose intensity is to be determined. It is therefore evident that one great thing to be aimed at is an absolutely uniform source of light. In the ordinary process of photometry the standard used is a candle, defined by Act of Parliament as a " sperm of six to the pound, burning at the rate of 120 grains per hour." This, however, is found to be very variable, and many observers have altogether condemned the employment of test-candles as light-measures.

The author has taken some pains to devise a source of light which should be at the same time fairly uniform in its results, would not vary by keeping, and would be capable of accurate imitation at any time and in any part of the world by mere description. The absence of these conditions seems to be one of the greatest objections to the sperm-candle. It would be impossible for an observer on the continent, ten or twenty years hence, from a written description of the sperm-candle now in use, to make a standard which would bring his photometric results into relation with those obtained here. Without presuming to say that he has satisfactorily solved all diffi- culties, the writer believes that he has advanced some distance in the right direction, and pointed out the road for further improvement.

A glass lamp is taken of about 2 ounces capacity, the aperture in the neck being $0\cdot25$ inch in diameter ; another aperture at the side allows the liquid fuel to be introduced ; this consists of alcohol

of sp. gr. 0·805, and pure benzol boiling at 81° C., which are mixed together in the proportion of five volumes of the former and one of the latter. The wick-holder consists of a platinum tube, and the wick is made of fifty-two pieces of platinum wire, each 0·01 inch in diameter. The flame of this lamp forms a perfectly shaped cone, the extremity being sharp, and having no tendency to smoke ; without flicker or movements of any kind, it burns when protected from currents of air at a uniform rate of 136 grains per hour.

There is no doubt that this flame is very much more uniform than that of the sperm-candle sold for photometric purposes. Tested against a candle, considerable variations in relative illuminating power have been observed ; but on placing two of these lamps in opposition, no such variations have been detected.

The instrument devised for measuring the relative intensities of the standard and other lights is next described ; it has this in common with that of Arago described in 1833, as well as with those described in 1853 by Bernard, and in 1854 by Babinet, that the phenomena of polarized light are used for effecting the desired end*. But it is believed that the present arrangement is quite new, and it certainly appears to answer the purpose in a way which leaves little to be desired. The instrument cannot be described without the aid of drawings, which accompany the original paper ; but its mode of action may be understood by the following description.

The standard lamp being placed on one of the supporting pillars which slide along a graduated stem, it is moved along the bar to a convenient distance, depending on the intensity of the light to be measured. The light to be compared is then fixed in a similar way on the other side of the instrument. On looking through the eyepiece two brightly luminous disks will be seen, of different colours. One of the lights must now be slid along the scale until the two disks of light, as seen in the eyepiece, are equal in tint. Equality of illumination is easily obtained ; for, as the eye is observing two adjacent disks of light which pass rapidly from *red-green* to *green-red*, through a neutral point of no colour, there is no difficulty in hitting this point with great precision. Squaring the distance between the flames and the centre will give inversely their relative intensities.

The delicacy of this instrument is very great. With two lamps, each about 24 inches from the centre, it is easy to distinguish a movement of one of them to the extent of one-tenth of an inch to or fro ; and by using the polarimeter an accuracy exceeding this can be attained.

The employment of a photometer of this kind enables us to compare lights of different colours with one another. So long as the observer, by the eyepiece alone, has to compare the relative intensities of two surfaces respectively illuminated by the lights under trial, it is evident that, unless they are of the same tint, it is impos-

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* Since writing the above, I have ascertained that M. Jamin had previously devised a photometer in which the principle adopted in the one here described is employed, although it is carried out in a different and, as I believe, a less perfect manner.—W. C., Dec. 16, 1868.

sible to obtain that absolute equality of illumination in the instrument which is requisite for a comparison. By the unaided eye one cannot tell which is the brighter half of a paper disk illuminated on one side with a reddish, and on the other with a yellowish light ; but by using the photometer here described the problem becomes practicable. When the contrasts of colour are very strong (when, for instance, one is a bright green and the other scarlet) there is difficulty in estimating the exact point of neutrality ; but this only diminishes the accuracy of the comparison, and does not render it impossible, as it would be according to other systems.

January 7, 1869.—Lieut.-General Sabine, President, in the Chair.

The following communication was read :—

"On the Mechanical Possibility of the Descent of Glaciers by their Weight only." By the Rev. Henry Moseley, M.A., Canon of Bristol, F.R.S., Instit. Imp. Sc. Paris, Corresp.

All the parts of a glacier do not descend with a common motion ; it moves faster at its surface than deeper down, and at the centre of its surface than at its edges. It does not only come down bodily, but with different motions of its different parts ; so that if a trans- verse section were made through it, the ice would be found to be moving differently at every point of that section.

This fact[*], which appears first to have been made known by M. Rendu, Bishop of Annecy, has since been confirmed by the mea- surements of Agassiz, Forbes, and Tyndall.   There is a constant dis- placement of the particles of the ice over one another, and alongside one another, to which is opposed that force of resistance which is known in mechanics as *shearing force.*

By the property of ice called regelation, when any surface of ice so sheared is brought into contact with another similar surface, it unites with it, so as to form, of the two, one continuous mass.   Thus a slow displacement of shearing, by which different similar sur- faces were continually being brought into the presence and contact of one another, would exhibit all the phenomena of the motion of glacier ice.

Between this resistance to shearing and the force, whatever it may be, which tends to bring the glacier down, there must be a mechanical relation, so that if the shearing resistance were greater the force would be insufficient to cause the descent.   The shearing

[*] The remains of the guides lost in 1820, in Dr. Hamel's attempt to ascend Mont Blanc, were found imbedded in the ice of the Glacier des Bossons in 1863. " The men and their things were torn to pieces, and widely separated by many feet. All around them the ice was covered in every direction for twenty or thirty feet with the hair of one knapsack, spread over an area of three or four hundred times greater than that of the knapsack." " This," says Mr. Cowell, from whose paper read before the Alpine Club in April 1864 the above quotation is made, " is not an isolated example of the scattering that takes place in or on a glacier, for I myself saw on the Theodule Glacier the remains of the Syndic of Val Tour- nanche scattered over a space of several acres."

force of cast iron, for instance, is so great that, although its weight is also very great, it is highly improbable a mass of cast iron would descend if it were made to fill the channel of the Mer de Glace, as the glacier does, because its weight would be found insufficient to overcome its resistance to shearing, and thus to supply the work necessary to those internal displacements, of which a glacier is the subject, or even to shear over the irregularities of the rocky channel. The same is probably true of any other metal.

I can find no discussion which has for its object to determine this mechanical relation between what is assumed to be the cause of the descent of a glacier, and the effect produced—to show that the work of its weight (supposing that alone to cause it to descend) is equal to the works of the several resistances, internal and external, which are actually overcome in its descent. It is my object to establish such a relation.

The forces which oppose themselves to the descent of a glacier are :—1st. The resistance to the sliding motion of one part of a piece of solid ice on the surface of another, which is taking place continually throughout the mass of the glacier, by reason of the different velocities with which its different parts move. This kind of resistance will be called in this paper (for shortness) *shear*, the *unit* of shear being the pressure in lbs. necessary to overcome the resistance to shearing of one square inch, which may be presumed to be constant throughout the mass of the glacier.

2ndly. The friction of the superimposed laminæ of the glacier (which move with different velocities) on one another, which is greater in the lower ones than the upper.

3rdly. The resistance to abrasion, or shearing of the ice, at the bottom of the glacier, and on the sides of its channel, caused by the roughnesses of the rock, the projections of which insert themselves into its mass, and into the cavities of which it moulds itself.

4thly. The *friction* of the ice in contact with the bottom and sides so sheared over or abraded.

If the whole mechanical *work* of these several resistances in a glacier could be determined, as it regards its descent, for any relatively small time, one day for instance, and also the *work* of its weight in favour of its descent during that day, then, by the principle of "virtual velocities" (supposing the glacier to descend by its weight only), the aggregate of the *work* of these resistances, opposed to its descent, would be equal to the work of its weight, in favour of it. It is, of course, impossible to represent this equality mathematically, in respect to a glacier having a variable direction and an irregular channel and slope ; but in respect to an imaginary one, having a constant direction and a uniform channel and slope, it is possible.

Let such a glacier be imagined, of unlimited length, lying on an even slope, and having a uniform rectangular channel, to which it fits accurately, and which is of a uniform roughness sufficient to tear off the surface of the glacier as it advances. Such a glacier would

descend with a uniform motion if it descended by its weight only, because the forces acting upon it would be uniformly distributed and constant forces*. The conditions of the descent of any one portion of it would therefore be the same as those of any other equal and similar portion. The portion, the conditions of whose descent it is sought in this paper to determine, is that which has descended through any given transverse section in a day ; or, rather, it is one half this mass of ice, for the glacier is supposed to be divided by a vertical plane, passing through the central line of its surface, it being evident that the conditions of the descent of the two halves are the same. The measurements which have been made of the velocities of the surface-ice at different distances from the sides make it probable that the differences of the spaces described in a given time would be nearly proportional to the distances from the edge in a uniform channel†, and the similar measurements made on the velocities at different depths on the sides that, under the same circumstances, the increments of velocity would be as the distances from the bottom. This law, which observation indicates as to the surface and the sides, is supposed to obtain throughout the mass of the glaciers. Any deviation from it, possible under the circumstances, will hereafter be shown to be such as would not sensibly affect the result.

The trapezoidal mass of ice thus passing through a transverse section in a day is conceived to be divided by an infinite number of equidistant vertical planes, parallel to the central line, or axis of the glacier, and also by an infinite number of other equidistant planes parallel to the bed of the glacier. It is thus cut into rectangular prisms or strips lying side by side and above one another. If any one of these strips be supposed to be prolonged through the whole length of the glacier, every part of it will be moving with the same velocity, and it will be continually shearing over two of the similar adjacent strips, and being sheared over by two others. The position of each of these elementary prisms in the transverse section of the glacier is determined by rectangular coordinates ; and in terms of these, its length, included in the trapezoid. The work of its *weight*, while it passes through the transverse section into its actual position, is then determined, and the work of its *shear*, and the work of its

* It is supposed that the weight is only just sufficient to cause the descent.

† Prof. Tyndall measured the velocity of the surface of the Mer de Glace at a series of points in the same straight line across it at a place called Les Ponts. The distances of these points in feet along the line up to the point of greatest velocity are set off to a scale in fig. 1 ; and the space in feet through which each point would pass in thirty-six days, if its velocity continued uniformly the same, is shown by a corresponding line at right angles to the other. The extremities of these last lines are joined. It will be seen that the line joining them is for some distance nearly straight ; if it were exactly so, the law stated in the text would, in respect to this ice, be absolutely true. Fig. 2 shows in the same manner the spaces described in thirty-six days by points at different depths on the side of the Glacier du Géant, as measured by Prof. Tyndall at the Tacul. See Phil. Trans. Royal Society, vol. cxlix. part 1, pp. 265, 266. [The figures referred to in this note accompany the MS. of the paper.]

friction. A double integration of each of the functions, thus representing the internal work in respect to a given elementary prism, determines the whole internal work of the trapezoid, in terms of the space traversed by the middle of the surface in one day, the spaces traversed by the upper and lower edges of the side, and a symbol representing the unit of *shear.* Well-known theorems serve to determine the *work* of the *shear* and the *friction* of the bottom and side in terms of the same quantities. All the terms of the equation above referred to are thus arrived at in terms of known quantities, except the unit of *shear,* which the *equation* thus determines. The comparison of this unit of *shear* (which is the greatest possible, in order that the glacier may descend by its weight alone) with the actual unit of *shear* of glacier ice (*determined by experiment*), shows that a glacier cannot descend by its weight only ; its shearing force is too great. The true unit of shear being then substituted for its symbol in the equation of condition, the work of the force, which must come in aid of its weight to effect the descent of the glacier, is ascertained.

The imaginary case to which these computations apply, differs from that of an actual glacier in the following respects. The actual glacier is not straight, or of a uniform section and slope, and its channel is not of uniform roughness. In all these respects the resistance to the descent of the actual glacier is greater than to the supposed one. But this being the case, the resistance to shearing must be less, in order that the same force, viz. the weight, may be just sufficient to bring down the glacier in the one case, as it does in the other. The ice in the natural channel must shear more easily than that in the artificial channel, if both descend by their weight only ; so that if we determine the unit of shear necessary to the descent of the glacier in the artificial channel, we know that the unit of shear necessary to its descent by its weight only in the natural channel must be *less than that.*

A second possible difference between the case supposed and the actual case lies in this, that the velocities of the surface-ice at different distances from the edge, and at different heights from the bottom, are assumed to be proportional to those distances and heights ; so that the mass of ice at any time passing through a transverse section may be bounded by plane surfaces, and have a trapezoidal form. This may not strictly be the case. All the measurements, however, show that if the surfaces be not plane, they are convex *downwards.* In so far therefore as the quantity of ice passing through a given section in a day is different from what it is supposed to be, it is greater than it. A greater resistance (other than shearing) is thus opposed to each day's descent, and also a greater weight of ice favours it ; but the disproportion is so great between the work of the additional resistance to the descent, and that of the additional weight of ice in favour of it, that it is certain that any such convexity of the trapezoidal surface would necessitate a further reduction of the unit of shear, to make the weight of the actual glacier sufficient to cause it to descend.

A third difference between the actual glacier and the imaginary one, to the computation of whose unit of shear the following formulæ are applied, is this—that the formulæ suppose the daily motion of the surface of the glacier and the daily motion of its side to have been measured at the same place, whereas there exist no measurements of the surface motion and the side motion at the same place. The surface motion used has been that of the Mer de Glace at Les Ponts, and the side motion that of the Glacier du Géant at the Tacul—both from the measurements of Prof. Tyndall. This error again, however, tends to cause the unit of shear, deduced from the case of the artificial glacier, to be greater than that in the actual one ; for the Glacier du Géant moves more slowly than the Mer de Glace. The quantity of ice which actually passes through a section at Les Ponts is therefore greater than it is assumed in the computation to be, whence it follows, as in the last case, that the computed unit of shear is greater than the actual unit of shear.

To determine the actual value of μ (the unit of shear in the case of ice) the following experiment was made. Two pieces of hard wood, each three inches thick and of the same breadth, but of which one was considerably longer than the other, were placed together, the surfaces of contact being carefully smoothed, and a cylindrical hole, $1\frac{1}{2}$ inch in diameter, was pierced through the two. The longer piece was then screwed down upon a frame which carried a pulley, over which a cord passed to the middle of the shorter piece, which rested on the longer. There were lateral guides to keep the shorter piece from deviating sideways when moved on the longer. The hole in the upper piece being brought so as accurately to coincide with that in the lower, small pieces of ice were thrown in, a few at a time, and driven home by sharp blows of a mallet on a wooden cylinder. By this means a solid cylinder of ice was constructed, accurately fitting the hole. Weights were then suspended from the rope, passing over the pulley until the cylinder of ice was sheared across. As by the melting of the ice, during the experiment, the diameter of the cylinder was slightly diminished, it was carefully measured with a pair of callipers.

1st experiment.—Radius of cylinder ·65625 in., sheared with 98 lbs.

2nd experiment.—Radius of cylinder ·70312 in., sheared with 119 lbs.

By the first experiment the shear per square inch, or *unit* of shear, was 72·433 lbs. ; by the second experiment it was 76·619 lbs. The main unit of shear of ice, from these two experiments, is therefore 75 lbs.

Now it appears by the preceding calculations, that to descend by its own weight, at the rate at which Prof. Tyndall observed the ice of the Mer de Glace to be descending at the Tacul, the unit of shearing force of the ice could not have been more than 1·3193 lb.*

* By an experiment on the shearing of putty, similar to that which was made on the shearing of ice, its unit of shear was found to vary from 1 lb. to 3 lbs., according to its degree of hardness. If ice were of the same weight per unit of

To determine how *great* a force, in addition to its weight, would be necessary to cause the descent of a glacier of uniform section and slope, such as has been supposed in the calculations, let u represent, in inch-lbs., the *work* of that force in twenty-four hours. Then assuming the unit of shear (μ) in glacier ice to be 75 lbs., it follows, by the principle of virtual velocities, that

$$u = 94134000 + 1012560 - 2668400$$
$$= 92478160 \text{ inch-lbs.} = 7706513 \text{ foot-lbs.*}$$

This computation has reference to half only of the width of the glacier, and to 23·25 inches of its length. The work, in excess of its weight, required to make a mile of the imaginary glacier, 466 yards broad and 140 feet deep, descend, as it actually does descend per twenty-four hours, is represented by the horse-power of an engine, which, working constantly day and night, would yield this work, or by

$$\frac{2 \times 7706513 \times 5280 \times 12}{23 \cdot 2 \times 24 \times 60 \times 33000} = 883 \cdot 78 \text{ h. p.}$$

The surface of the mass of ice, on which the work u is required to be done, in aid of its weight, to make it descend as it actually does, is 124771·5 square inches. The work required to be done on each square inch of surface, supposing it to be equally distributed over it, is therefore, in foot-lbs., $\dfrac{7706513}{124771 \cdot 5} = 61 \cdot 76$.

These 61·76 foot-lbs. of work are equivalent to ·0635 heat-units, or to the heat necessary to raise ·0635 lb. of water by one degree of Fahrenheit. This amount of heat passing into the mass of the glacier per square inch of surface per day, and reconverted into mechanical work *there*, would be sufficient, together with its weight, to bring the glacier down.

The following considerations may serve to disabuse some persons of the idea of an unlimited reservoir of force residing somewhere in the prolongation of a glacier backward, and in its higher slopes, from which reservoir the pressure is supposed to come which crushes the glacier over the obstacles in its way.

Let a strip of ice one square inch in section, and one mile in length, in the middle of the surface of the imaginary glacier, be conceived to be separated from the rest throughout its whole length, except for the space of one inch, so that throughout its whole length, except for that one inch, its descent is not retarded either by shear or by friction. Let, moreover, this inch be conceived to be at the very end of the glacier, so that there is no glacier beyond it. Now it may easily be calculated that this strip of ice, one inch square and one mile long, lying on a slope of 4° 52', without any resistance to

volume as soft putty, and its consistency about the same, it would descend by its weight *only*, without the aid of any other force. It would not, however, be possible to walk on such ice.

* Thus the work to be done in aid of the weight is thirty-four times the work of the weight.

its descent, except at its end, must press against its end, by reason of its weight, with a force of 194·42 lbs. But the cubical inch of solid ice at its extremity opposes, by the *shear* of its three surfaces whose attachment to the adjacent ice is unbroken, a resistance of 3×75 lbs., or 225 lbs. That resistance stops therefore the descent of this strip of ice, one mile long, having no other resistance than this opposed to its descent, by reason of its detachment from the rest*. It is clear, then, that it could not have descended by its weight only when it *adhered* to the rest, and when its descent was opposed by the shear of its whole length ; and the same may be proved of any number of miles of strip in *prolongation* of this. Also, with obvious modifications, it may be shown, in the same way, to be true of any *other* similar strip of ice in the glacier, whether on the surface or not, and therefore of the whole glacier.

It results from this investigation that the weight of a glacier is insufficient to account for its descent ; that it is necessary to conceive, in addition to its weight, the operation of some other and much greater force, which must also be such as would produce those internal molecular displacements and those strains which are observed actually to take place in glacier ice, and must therefore be present to every part of the glacier as its weight is, but more than thirty-four times as great.

XXXII. *Intelligence and Miscellaneous Articles.*

FORMATION OF AN ARTIFICIAL SPECTRUM WITH ONE FRAUNHOFER'S LINE. BY A. WÜLLNER.

ACCORDING to Kirchhoff's explanation of Fraunhofer's lines, they are formed by an absorption in the ignited gas surrounding the solid core of light of particular wave-lengths emitted from the core. If the solar atmosphere alone sent us light, these lines, assuming that the intensity of the light proceeding from the atmosphere was great enough, must appear bright upon a dark ground. This inversion may possibly be observed at the total eclipse of next August.

I may be allowed to communicate an experiment which shows the phenomenon with a terrestrial source of light, in the same manner as Kirchhoff has concluded for the sun from the absorption of light in flames.

If the discharges of a Leyden jar of about a square foot of coated surface, and a small striking-distance, be passed through a Geissler's spectrum-tube of the usual form by the aid of a Holtz's machine, the tube being held before the slit of a spectrometer, the spectrum of the gas enclosed in the tube is first seen as on the passage of a powerful induction-current. If the striking-distance be only a little in-

* If, however, the glacier were inclined at 35° 10′, instead of 4° 52′, and a strip were detached from its surface, as described above, it would equal the shear of one cubic inch at its lower end, if it were 300 yards long, and if the glacier were vertical, when it was 172·8 yards long.

creased, the sodium-line is added to the spectrum of the gas, as it is seen that while using the induction-current the capillary part of the spectrum-tube in front of the slit is heated. With a suitable striking-distance, the sodium-line is so bright that it far exceeds in intensity the lines of the gas-spectrum if a hydrogen-spectrum tube has been taken. If the striking-distance be slightly increased, the bright lines of the calcium-spectrum occur with a beauty and precision which can scarcely be obtained in any other way. Passing beyond the striking-distance which has furnished this spectrum, the entire phenomenon changes. The luminous line in the spectrum-tube is of such dazzling brightness, that even looked at by daylight it leaves a long-continued after-image in the eye. This luminous line, looked at in the spectrometer, shows a considerably bright continuous spectrum, in which, however, the position of the sodium-line appears quite dark ; we thus obtain an artificial spectrum with *one* dark line, or, since the mode of formation is the same, with *one* Fraunhofer's line.

That this line is formed in the same manner as, according to Kirchhoff, Fraunhofer's are in the solar spectrum, is at once seen on looking at the tube after the experiment ; the inside of the capillary tube is seen to be very strongly corroded by detached glass splinters, so that after the experiment has been often made the glass has become quite dull. These glass particles, which each discharge detaches, are at the same time raised to incandescence, and the light of these ignited solid particles furnishes the bright continuous spectrum. These solid particles glow, however, in an atmosphere of sodium-vapour, and this holds back the same light which was observed before the solid particles were detached ; hence where this light ought to be in the spectrum there is a dark part. There is here seen in its individual stages the formation of Fraunhofer's line ; for the discharge of the Leyden jar first produces the ignited atmosphere, and then the far more brightly glowing core in it. With the formation of the glowing solid core the bright line furnished by the atmosphere becomes obscure.

It might be expected that, besides the sodium-line, the calcium-line and that of the gas would appear dark. I have been unable to perceive these lines ; the reason doubtless is that calcium-vapour is not sufficiently dense to allow the absorption of the light corresponding to it to be so strong that the inversion can occur.—Poggendorff's *Annalen*, No. 9, 1868.

ON THE QUANTITY OF ELECTRICITY PRODUCED BY THE ELECTROPHORUS MACHINE EXPRESSED IN ABSOLUTE MEASURE. BY F. KOHLRAUSCH.

The working of Holtz's electrophorus machine has been investigated by means of the unit-jar, and particularly as compared with that of the friction machine. An absolute measurement in this way would be attended with the greatest difficulties. If we restrict our-

selves to the limiting case in which the conductors are in metallic contact (which in some cases might possibly be applied instead of voltaic electricity), the quantities of electricity produced are easily obtained in absolute measure if the *magnetic actions* of the current they form are observed. The reduction of the measurements of the intensity of the current to mechanical units, which my father and Weber executed, renders it easy to express quantities of electricity in electrostatic units.

The machine investigated was constructed by Schultz of Berlin, according to the pattern described by H. Holtz in vol. xxx. of this Magazine. The fixed disk has two coatings; the moveable one has a diameter of 400 millims. Where it is not otherwise expressly mentioned, only two of the four combs which were opposite the coatings were at work.

By velocity of rotation is understood the number of turns of the handle in a second, one of which corresponds to six turns of the glass disk.

The two conductors were connected (a moist thread being interposed) with the ends of the wire of the same multiplier as was arranged for the research of Weber and my father, and which is described in the fifth volume of the Proceedings of the Royal Academy of Saxony, pp. 259 & 289. The 5635 turns of its wire, amounting to about three miles, were completely protected against the formation of short circuit by careful insulation.

The intensity of the current which flows through the coils of this multiplier and deflects the needle through ϕ, is in absolute magnetic measure

$$i = \frac{\text{T}}{262 \cdot 1} \log \phi,$$

or, with the present value of the horizontal intensity of terrestrial magnetism for Göttingen $\text{T} = 1 \cdot 844$,

$$i = 0 \cdot 00704 \log \phi.$$

In what follows, the distance of the scale from the mirror of the magnet was $= 1400$ divisions; hence, if p represents the deflection of the needle in parts of the scale, for small values of p we can put

$$i = \frac{0 \cdot 00704}{2800} p = 0 \cdot 00000251 \cdot p.$$

The very powerful damper completely stopped the needle after a few minutes; and with a little practice it was possible so to regulate the velocity of rotation by a seconds' watch, that the deviations only amounted to a few divisions.

It was surprising to find the working of the machine *as good as constant* on different days. Although the absolute moisture of the air in the room of observation was between 9 and 14 grms. of water in a cubic metre, and the relative varied between $0 \cdot 42$ and $0 \cdot 58$, and the greatest length of spark was subject to great variations, as is usual with these machines, yet the inequalities observed in the in-

tensity of the current were within the errors of observation. If, for instance, the mean be taken of the sets of observations on the 16th, 17th, and 18th of July, and those on the 25th and 26th of July, the deflections, in parts of a scale, are—

Velocity of rotation .... $= \frac{1}{3}$	$\frac{1}{2}$	$\frac{1}{1}$	$\frac{3}{2}$
16th, 17th, and 18th of July $=30\cdot2$	$42\cdot6$	$89\cdot2$	$137\cdot0$
25th and 26th of July .... $=29\cdot7$	$43\cdot8$	$87\cdot2$	$137\cdot0$
Mean.. .... $=30\cdot0$	$43\cdot1$	$88\cdot8$	$137\cdot0$

The differences are similarly small if the mean be taken of other groups.

It is, secondly, remarkable that the electricity furnished is, within the limits prescribed by the dimensions of the machine, as good as independent of the distance between the combs and the rotating plate, as follows from the following numbers, which also represent the mean values of several experiments :—

Velocity of rotation		$= \frac{1}{3}$	$\frac{1}{2}$	$\frac{1}{1}$	$\frac{3}{2}$
Distance of the combs $\lbrace$	4 millims.	$30\cdot5$	$45\cdot2$	$90\cdot7$	$139\cdot4$
	19 ,,	$30\cdot0$	$46\cdot0$	$89\cdot2$	$131\cdot7$
from the glass disk	27 ,,	$30\cdot5$	$47\cdot0$	$88\cdot2$	$130\cdot0$
	34 ,,	$30\cdot9$	$46\cdot0$	$87\cdot1$	$130\cdot3$

The working with a slow rotation remained unchanged ; with the greatest velocity the diminution is only about 7 per cent. Hence for a velocity of the particles of glass of 3 metres in a second, the discharge and charge are as complete when the distance of the points is 34 millims. as it is when it is 4 millims. With a velocity of 6 to 9 metres the discharge would only be incomplete—a point which, like any motion of electricity for which a measurable time is required, would deserve further examination. (The above observations are less trustworthy, owing to the greater variations which always accompanied the further removal of the combs. A few times a sudden decrease in activity set in which extended to total cessation.)

It is seen, thirdly, that there is an almost *exact proportionality between the intensity of the current and the velocity of rotation*, for which purpose it is sufficient to compare the numbers given above. After introducing a small correction for magnitudes which are proportional to the tangent of the angle of deflection, we have, in the mean,—

Velocity of rotation .... $= \frac{1}{3}$	$\frac{1}{2}$	$\frac{1}{1}$	$\frac{3}{2}$
Deflection .... $=30\cdot0$	$43\cdot1$	$88\cdot7$	$136\cdot6$

while, assuming proportionality, we calculate

Deflection .... $=29\cdot9$	$44\cdot8$	$89\cdot7$	$134\cdot5$

The productiveness of Holtz's machine was further compared with that of a very good *friction machine* (made on Winter's plan by Apel) which has a disk of 600 millims., and under favourable cir-

cumstances yields sparks of 15 inches. Here also the quantity of electricity was almost proportional to the velocity of rotation; for—

Velocity of rotation	$=\frac{1}{3}$	$\frac{1}{2}$	$\frac{1}{1}$	$\frac{3}{2}$
Deflection	$=9\cdot7$	$14\cdot7$	$26\cdot7$	$40\cdot5$
Calculated	$=9\cdot2$	$13\cdot7$	$27\cdot5$	$41\cdot2$

As both machines can be turned with the same velocity, Holtz's is more productive in the ratio 10 : 3.

In reducing the current to *absolute magnetic measure*, the deflection must be multiplied with $0\cdot00000251$. Owing to agitations, the greatest velocity observed, of three turns in two seconds, would be almost the maximum for continuous use with the above machine. If instead of the deflection 137 we take as the maximum 150 divisions, the intensity of the current in magnetic measure is

$$150 \times 0\cdot00000251 = 0\cdot000376.$$

Changed into *mechanical measure,* we have

$$0\cdot000376 \times 155370 \times 10^6 = 58 \text{ million}$$

units of positive electricity in a second, or a little more than one charge of the small Leyden jar used in the measurements of Weber and my father.

Yet how small this quantity is as compared with that set in motion by voltaic currents is seen from the fact that the current $0\cdot000376$ only decomposes $\frac{3\cdot5}{1000000}$ *of a milligramme of water,* or that, in order to obtain a cubic centimetre of explosive mixture, the strongest current of our electrophorus machine would have to be passed for about forty hours through water.

I have incidentally *found the electromotive force of a Grove's element in absolute measure to be* $= 17\cdot9$, assuming the magnetic as unit of current-intensity, and Siemens's as unit of resistance. H. von Waltenhofen* finds, expressed in the same unit, Daniell$=11\cdot4$; and Bosscha in the same manner finds Daniell$=10\cdot8$; from which $\frac{\text{Grove}}{\text{Daniell}} = 1\cdot57$ or $1\cdot66$ respectively, which is about the number obtained by direct comparison. The current $0\cdot000376$ of our machine is therefore produced by *a single Grove's* element in a conduction of $\frac{17\cdot9}{0\cdot000376} = 48000$ mercury-units, or in a telegraphic communication of about 3800 miles.

To obtain an idea of possible medical or physiological applications, I determined *the resistance of the human body,* which from hand to hand (the hands being immersed in dishes containing liquid) in four persons amounted to between 1600 and 3600 mercury-units, the mean being 2200. Putting the polarization as equal to the electromotive force of one Grove's element, which for feeble currents is far too high, a battery of two elements would furnish the current

* Pogg. *Ann.* vol. cxxxiii. p. 462.

$\frac{17\cdot9}{2200} = 0\cdot0081$, or *twenty-two times* the current produced by the machine in the human body.

Such numbers lead to the conclusion that, though far more productive machines than the above may, and probably will, be constructed by increased velocity of rotation, a larger number of places of decomposition, and of combs, yet they cannot be thought of as a substitute for *constant currents*, or in general for actions which are proportional to the intensity. It is different with actions which are proportional to the square of the intensity, like the thermal actions observed by Holtz and Poggendorff, or with the physiological which are produced by intermittent currents, and depend upon the rapidity of the change of current. By the possibility of resolving the mean current of the machine, by means of an interposed layer of air, into parts of greatly increased intensity, actions may be obtained which, under certain circumstances, may approach to galvanic ones.—Poggendorff's *Annalen*, No. 9, 1868.

ON THE INTERFERENCE OF LIQUID WAVES.
BY M. LISSAJOUS.

The interference of liquid waves may be demonstrated by means of an apparatus which is both simple and easy to construct. It consists of two horizontal diapasons fixed to the same support in such a manner that the four branches are in the same horizontal plane. Each diapason is provided with a vertical point; these two dip at a distance of a few centimetres in a bath of mercury or water.

If one of them be made to vibrate, the disturbances communicated to the liquid produce waves which are propagated with greater or less rapidity, according to the nature of the liquid. If both be simultaneously disturbed, a system of fixed waves is formed between the two points which are perfectly distinguishable by the play of light which they produce.

When the two diapasons are not in perfect unison, the waves are seen to be slowly displaced; and it can readily be proved that they move towards the lowest one.

Thus, by means of the eye, not merely the discordance of two vibrating apparatus may be recognized, but also its direction.

This second experiment furnishes an ocular demonstration of the cause of beats.

It is also possible to use this method for comparing the velocity of the propagation of waves on the surface of different liquids. —*Comptes Rendus*, December 24, 1868.

XXXIII. *On Cometary Theory.*
By JOHN TYNDALL, *F.R.S. &c.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

ON the 8th of this month, in a lecture delivered before the Cambridge Philosophical Society, I ventured to enunciate a speculation regarding the origin and deportment of visible cometary matter. I had been led to reflect on the subject by those experiments, on the decomposition of vapours by light, which I have already described in abstract in the Proceedings of the Royal Society. The speculation was introduced and communicated to the Philosophical Society in the following words :—

"In the course of my experiments on actinic action I have been often astonished at the body of light which a perfectly infinitesimal amount of matter, when diffused in the form of a cloud, can discharge from it by reflection. I have been repeatedly perplexed and led into error by the action of residues so minute as to be simply inconceivable. In order to get rid of these residues, my experimental tubes, after having been employed for any vapour, are flooded with alcohol, sponged out with soap and hot water, and finally flooded with pure water. Let me give you some idea of the quantities of matter that here come into play. The tube before you, which is 3 feet long and 3 inches wide, was so thoroughly cleansed that when filled with air, or with the vapour of aqueous hydrochloric acid, no amount of exposure to an intense light produced the least cloudiness. Having thus assured myself of the perfect purity of the tube, I took a small

bit of bibulous paper, rolled it up into a pellet not the fourth part of the size of a small pea, and moistened it with a liquid possessing a higher boiling-point than that of water. I held the pellet in my fingers until it had become almost dry, then introduced it into a connecting-piece and allowed dry air to pass over it into this tube. The air charged with the modicum of vapour thus taken up was subjected to the action of light. A blue actinic cloud began to form immediately, and in five minutes the blue colour had extended quite through the experimental tube. For some minutes this cloud continued blue, and could be completely quenched by a Nicol's prism, no trace of its light reaching the eye when the Nicol was in its proper position. But its particles augmented gradually in magnitude, and at the end of fifteen minutes a dense white cloud filled the tube. Considering the amount of the vapour carried in by the air, the appearance of a cloud so massive and luminous seemed like the creation of a world out of nothing.

" But this is not all ; the pellet of bibulous paper was removed, and the experimental tube was cleared out by sweeping a current of dry air through it. *This current passed also through the connecting-piece in which the pellet of bibulous paper had rested.* The air was at length cut off and the experimental tube exhausted. Fifteen inches of hydrochloric acid were then sent into the tube through the same connecting-piece. Now it is here to be noted (1) that the total quantity of liquid absorbed by the pellet in the first instance was exceedingly small, (2) that nearly the whole of this small quantity had been allowed to evaporate between my fingers before the pellet was placed in the connecting-piece, (3) that the pellet had been ejected and the tube in which it rested rendered for some minutes the conduit of a strong current of pure air. It was part of such a residue as could linger in the connecting-piece after this process, that was carried into the experimental tube by the hydrochloric acid and subjected there to the action of light.

" One minute after the ignition of the electric lamp a faint cloud showed itself ; in two minutes it had filled all the anterior portion of the tube and stretched a considerable way down it ; it developed itself afterwards into a very beautiful cloud-figure ; and at the end of fifteen minutes the body of light discharged by the cloud, considering the amount of matter involved in its production, was simply astounding. But though thus luminous, the cloud was far too fine to dim in any appreciable degree objects placed behind it. The flame of a candle seemed no more affected by it than it would be by a vacuum. Placing a page of print so that it might be illuminated by the cloud itself, it could be read *through* the cloud without any sensible enfeeblement.

Nothing could more perfectly illustrate that ' spiritual texture' which Sir John Herschel ascribes to a comet than these actinic clouds. Indeed the experiments prove that matter of almost infinite tenuity is competent to shed forth light far more intense than that of the tails of comets. The weight of the matter which sent this body of light to the eye would probably have to be multiplied by millions to bring it up to the weight of the air in which it hung.

" And now will you bear with me for five minutes while I en- deavour to apply these results to cometary theory ? I am encou- raged to do so by a remark of Bessel's, who said that had any theory preceded his observations on Halley's comet, by fixing his attention either upon its verification or its confutation, it would have enabled him to return from his observations with a greater store of knowledge than he had actually derived from them. If time permitted, I should like to lead you by an easy gradient up to the view that I wish to submit to you; but time does not permit of this, and therefore the speculation must suffer from the baldness arising from the absence of such preparation.

" You are doubtless aware of the tremendous difficulties which beset cometary theory. The comet examined by Newton in 1680 shot out a tail sixty millions of miles in length in two days. The comet of 1843, if I remember aright, shot out in a single day a tail which covered 100 degrees of the heavens. This enormous reach of cloudy matter is supposed to be generated in the head of the comet and driven backwards by some mysterious force of repulsion exerted by the sun. Bessel devised a kind of magnetic polarity and repulsion to account for it. ' It is clear,' says Sir John Herschel, ' that *if we have to deal here with matter such as we conceive it, viz. possessing inertia, at all,* it must be under the dominion of forces incomparably more energetic than gravitation, and quite of a different nature.' And in another place he states the difficulties of the subject in the following re- markable words :—

" ' There is beyond question some profound secret and mystery of nature concerned in the phenomenon of their tails. Perhaps it is not too much to hope that future observation, borrowing every aid from rational speculation, grounded on the progress of physical science generally (especially those branches of it which relate to the æthereal or imponderable elements), may ere long enable us to penetrate this mystery, and to declare whether it is really *matter,* in the ordinary acceptation of the term, which is projected from their heads with such extravagant velocity, and if not impelled, at least *directed* in its course by a reference to the sun as its point of avoidance. In no respect is this ques- tion as to the materiality of the tail more forcibly pressed on us

for consideration than in that of the enormous sweep which it makes round the sun *in perihelio*, in the manner of a straight and rigid rod, in defiance of the law of gravitation, nay, even of the received laws of motion, extending (as we have seen in the comets of 1680 and 1843) from near the sun's surface to the earth's orbit, yet whirled round unbroken—in the latter case through an angle of 180° in little more than two hours. It seems utterly incredible that in such a case it is one and the same material object which is thus brandished. [I would especially invite the reader's attention to these words in reference to the following theory.—J. T.] If there could be conceived such a thing as a *negative shadow*, a momentary impression made upon the luminiferous æther behind the comet, this would represent in some degree the conception such a phenomenon irresistibly calls up.'

" I now ask for permission to lay before you a speculation which seems to do away with all these difficulties, and which, whether it represents a physical verity or not, ties together the phenomena exhibited by comets in a remarkably satisfactory way.

" 1. The theory is, that a comet is composed of vapour decomposable by the solar light, the visible head and tail being an actinic cloud resulting from such decomposition ; the texture of actinic clouds is demonstrably that of a comet.

" 2. The tail, according to this theory, is not projected matter, but matter precipitated on the solar beams traversing the cometary atmosphere. It can be proved by experiment that this precipitation may occur either with comparative slowness along the beam, or that it may be practically momentary throughout the entire length of the beam. The amazing rapidity of the development of the tail would be thus accounted for without invoking the incredible motion of translation hitherto assumed.

" 3. As the comet wheels round its perihelion, the tail is not composed throughout of the same matter, but of new matter precipitated on the solar beams, which cross the cometary atmosphere in new directions. The enormous whirling of the tail is thus accounted for without invoking a motion of translation.

" 4. The tail is always turned from the sun for this reason :— Two antagonistic powers are brought to bear upon the cometary vapour,—the one an *actinic* power tending to produce precipitation, the other a *calorific* power tending to effect vaporization. Where the former prevails, we have the cometary cloud ; where the latter prevails, we have the transparent cometary vapour. As a matter of fact, the sun emits the two agents here invoked. There is nothing whatever hypothetical in the assumption of their existence. That precipitation should occur behind the head of the comet, or in the space occupied by the head's

shadow, it is only necessary to assume that the sun's calorific rays are absorbed more copiously by the head and nucleus than the actinic rays. This augments the relative superiority of the actinic rays behind the head and nucleus, and enables them to bring down the cloud which constitutes the comet's tail.

" 5. The old tail, as it ceases to be screened by the nucleus, is dissipated by the solar heat; but its dissipation is not instantaneous. The tail leans towards that portion of space last quitted by the comet, a general fact of observation being thus accounted for.

" 6. In the struggle for mastery of the two classes of rays a temporary advantage, owing to variations of density or some other cause, may be gained by the actinic rays even in parts of the cometary atmosphere which are unscreened by the nucleus. Occasional lateral streamers, and the apparent emission of feeble tails towards the sun would be thus accounted for.

" 7. The shrinking of the head in the vicinity of the sun is caused by the beating against it of the calorific waves, which dissipate its attenuated fringe and cause its apparent contraction.

" Throughout this theory I have dealt exclusively with true causes, and no agency has been invoked which does not rest on the sure basis either of observation or experiment. It remains with you to say whether in venturing to enunciate it I have transgressed the limits of 'rational speculation.'

" If I have done so, surely I could not have come to a place more certain to ensure my speedy correction. If the theory be a mere figment of the mind, your Adams and your Stokes (both happily here present), to whom I submit the speculation with the view of having it instantly annihilated by astronomy and physics, if it merit no better fate, will, I doubt not, effectually discharge that duty, and thus save both you and me from error before it has had time to lay any serious hold on our imagination."

The leniency with which this speculation was received at Cambridge induces me to submit it to the critical judgment of the readers of the Philosophical Magazine*.

<div style="text-align:center">I remain, Gentlemen,</div>

<div style="text-align:center">Your obedient Servant,</div>

Royal Institution,

March 15, 1869.   JOHN TYNDALL.

* There may be comets whose vapour is undecomposable by the sun, or which, if decomposed, is not precipitated. This view opens out the possibility of invisible comets wandering through space, perhaps sweeping over the earth and affecting its sanitary condition without our being otherwise conscious of their passage. As regards tenuity, I entertain a strong persuasion that out of a few ounces (the possible weight assigned by Sir John Herschel to certain comets) of iodide-of-allyl vapour, an actinic cloud of the magnitude and luminousness of Donati's comet might be manufactured.

XXXIV. *Remarks on the Luminous, Thermal, and Acoustic Phenomena attending the Fall of Meteorites. By* Chevalier W. von HAIDINGER*.

1. *METEORIC falls in the course of the year* 1868.—The first of these falls in 1868 brought to the notice of the scientific world was that at Pultusk, near Warsaw, January 30. Next to it came the falls of Villanova and Motta dei Conti, near Casale (Piedmont), February 26, between 10.30 and 10.45 a.m., of Slavetic (Croatia), May 22, and of Ornans (Département du Doubs), July 11. The detonation of a fifth meteor was heard at Salzburg in the night of September 27–28, without having (at least as far as hitherto known) left any tangible traces. A fragment of the Ornans meteorite, weighing 39 grms., has come into possession of the Imperial Museum of Vienna through the kind intervention of the eminent geologist, M. Joachim Barrande. The fall of Villanova was preceded by a number of detonations; no igneous globe, only a small cloud with internal motion and in rapid progression, was observed. The meteor passed from N.W. or W.N.W. to S.E. or E.S.E. at a small angle to the horizon. Two stones, the one weighing 6·700 grms., the other 1·920 grm., have been preserved; a third, whose weight may have amounted to from 300 to 500 grms. was broken by the violence of its fall.

2. M. Stanislas Meunier has published '*Géologie comparée*: *Etude descriptive théorique et expérimentale sur les Météorites,*' Paris, 1867, a book highly commended by Madame Cat. Scarpellini. In one passage (p. 18) the author says, " A great number of circumstances attending meteoric falls have as yet remained without explanation. The causes of the explosions, especially when repeated, and of the incandescence, are still unknown." So it may be inferred that he had no knowledge of the notices on these questions published by von Haidinger in the Proceedings of the Vienna Academy (vol. xliii. p. 380, vol. xlvii. sect. 2. pp. 283–298). M. Meunier, quoting the *Nouvelles Archives du Muséum*, vol. iii. p. 1, 1867, says, further, (p. 29), " Generally the crust is not uniformly spread over the whole surface of the meteorites, showing seams and wrinkles, the form of which in certain cases may indicate the position kept by the meteorite during its flight through the air. These circumstances have been duly noticed by M. Daubrée in his remarks on the Meteorite of Orgueil." The late Director Schreibers, in a book published in 1820, and von Haidinger, in several papers published in the Vienna Academy's Proceedings (vol. xi. p. 525, and vol. xlv. p. 791), had, long before Professor Daubrée, de-

* Abstract of a memoir read before the Imperial Academy of Vienna, Oct. 8, 1868, for which we are indebted to Count Marschall, For. Corr. G.S.

scribed, figured, and discussed these " seams and wrinkles " as they appear on the crust of the meteorites of Stannern (Moravia, 1808).

3. Professor A. Daubrée has greatly added to our knowledge of meteorites, both by enlarging the collection of the Muséum d'Histoire Naturelle (somewhat neglected in this respect previously*), and by his theoretical investigations on this subject, especially by experiments concerning the artificial production of meteoroid substances. He has lately published his notices on the meteoric fall of Orgueil (May 14, 1864), communicated to the Paris Academy at eight meetings, between May 16 and November 14, 1864, together with notices on those of Tourinne-la-Grosse (December 7, 1863), of Aumale in Algeria (August 25, 1865), of St. Mesmin (May 30, 1866), and of Setif in Algeria (June 9, 1867). It must, however, be remarked that the learned Professor of the Jardin des Plantes never mentions von Haidinger's observations or views about meteorites, except (p. 8) where he quotes, *verbo tenus,* a passage concerning the means of discerning the position of meteorites during their course through cosmical space.

4. *Comparison of theoretical views.*—Von Haidinger distinguishes two periods in the existence of any meteorite : (1) from its primordial formation down to the *completion* of the body entering the terrestrial atmosphere ; (2) from its entering into the atmosphere down to its *fall* on the earth's surface. This last period, being the subject of *direct observation,* must be especially treated here, although the former, still to be studied in many respects, cannot be left entirely out of discussion.

5. A. *Phenomena connected with meteorites.*—No qualities of matter or forces except those known at present can be admitted in explaining these phenomena. As early as March 14, 1861, von Haidinger distinguished the consecutive phases as follows :—

(1) A fragment pursuing its way meets the upper limit of the terrestrial atmosphere.

(2) Its cosmical velocity meets a resistance, and, in consequence, suffers diminution.

(3) Meantime pressure developes light and heat, the meteorite rotates, and an enamelled crust forms on its surface.

(4) The heated stratum of air agglomerates to an " igneous globe " behind the meteor.

(5) The meteor having reached the term of its cosmic course becomes stationary.

(6) The development of light and heat ceases ; the surround-

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* On December 15, 1863, the Museum specimens numbered 86 from different localities ; on March 31, 1868, the number had increased to 203.

ing air suddenly rushing into the vacuum within the igneous
globe causes a detonation.

(7) A compensation of temperature takes place between the
cold internal nucleus and the external surface.

(8) The meteorite, now subject to the laws of gravitation,
falls on the surface, its temperature standing in direct proportion
to the thermo-conductive power of its substance.

Ad 1. *A fragment in its course meets the upper limit of the
terrestrial atmosphere.*—The existence of but one single frag-
ment detached from a larger body is supposed, in order to
simplify the reasoning. Two or more fragments moving in the
same direction under the impulse of the same force, either
in immediate contact or with a greater or less distance be-
tween them, may as well be supposed to enter at once the ter-
restrial atmosphere. Professor Daubrée expresses a different
opinion, saying (p. 16), " Nothing entitles us to admit, as it has
been asserted, that the different meteorites of the same fall tra-
verse cosmical space separated from each other and, as it were,
in swarms ; " and (p. 14), " So in general the bolides leave
us parts of their substance in three different ways : (1) by
combustion, vaporization, or gasification ; (2) by being dis-
persed into dust, the fall of which generally escapes observation,
and which may be analogous to the dust gathered after certain
meteoric falls ; (3) by dispersion of the fragments detached at
the moment of the explosion." Further, " The proceedings are
such as if the greatest portion of the meteoric mass would pass
from the atmosphere to continue its cosmical course, leaving be-
hind only some particles whose velocity has been lessened in
consequence of the explosion." Professor Daubrée admits still
another supposition, saying (p. 10), " There is no proof that,
besides this sudden exhalation, these small planetary bodies are
not endowed with an atmosphere of their own, which indeed
they absorb soon after their entrance into our atmosphere.
These gaseous substances, whatever may be their origin, may, by
their combustion, increase the splendour of the meteor ; how-
ever, as they would burn away in continuity, they *leave unex-
plained the fact of the explosion* preceding any fall of meteo-
rites." If Professor Daubrée, having entered into the most mi-
nute distinctions and called in to aid several very objectionable
suppositions, concludes with acknowledging the want of satis-
factory explanations for the explosions, it must not be wondered
at that M. St. Meunier speaks of their cause as " absolutely un-
known." In fact, an " explosion " exists not, and therefore
neither admits nor wants explanation. The term itself is im-
proper ; and if instead of it the term " detonation " is used (as
it ought to be), an inaccurate presupposition is eliminated and

explanation made easier. The first proposition may be expressed more precisely thus : *A fragment (or a group of fragments) in its course meets the limits of the terrestrial atmosphere.*

Ad 2. *The cosmical velocity of the fragments meets resistance, and consequently is diminished.*—This proposition is certainly not subject to any objection.

Ad 3. *Together with the diminution of velocity, the compression of the air developes light and heat, the meteorite takes a rotatory movement, and an enamelled crust forms on its surface.*

Ad 4. *The heated stratum of air, due to the pressure of the meteorite deprived of its cosmical velocity, is pushed forward in proportion to the initial force of movement, and agglomerates behind it in an " igneous globe."*

a. *Luminous Phenomena.*—The ideal diagrams (figs. 1 and 2) may assist in the explanation of these phenomena. Pressure produces a centre of expansion in front of the meteorite. The com-

Fig. 1.
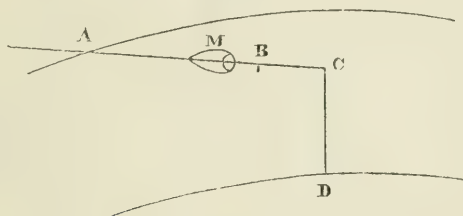
Fig. 2.
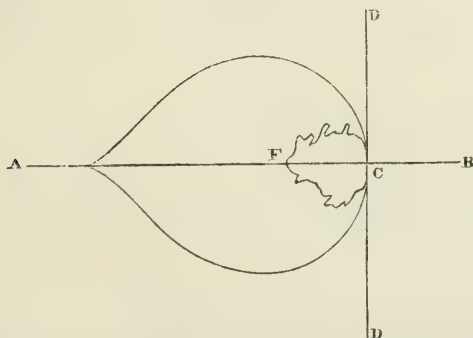
pressed air is forced out of it radially in every direction. The resistance remains equal against this luminous disk ; only at some distance in the direction of the line of course it overcomes the luminous and incandescent stratum of air, pressing it at last behind the course of the meteorite. *Movement is transmuted into light and heat*—a proposition frequently enunciated of late years, and

perfectly applicable to the process here in question. The celebrated Professor G.V. Schiaparelli announces it in these terms: "While the meteoric substance is dispersed through the atmosphere, this *vis viva*, being transformed into heat and light, is entirely destroyed." This explanation is rejected by Professor Daubrée, who says (p. 8), "The recent investigations as to the mechanical production of heat have induced some to suppose the heat of meteorites to be the consequence of their loss of *vis viva*. This supposition is in contradiction to the *carbonaceous* meteorites, which, instead of being hot as far as their centre, as they ought to be according to this supposition, include substances liable to be altered or dispersed by a slight increase of temperature." He has not, however, considered this transformation as proceeding not suddenly, but by *degrees*, in proportion to the atmospheric resistance, and only on the surface where the air is in contact with the meteoric nucleus. He supposes likewise the meteorites to come cold out of cosmical space on account of their composition being easily destructible by heat, although he does not mention the extremely low temperature which must be supposed to exist in cosmical space. The learned Professor, when ascribing (p. 9) the thermal phenomena observed in meteorites to the development of *electricity* provoked by violent friction, is rather commenting on than contradicting von Haidinger's views.

It may be a question what idea is to be conceived of the dispersion of a swarm of meteors, different in size, without detonation and at the moment of quietly entering the atmosphere. Director Julius Schmidt has witnessed this phenomenon at Athens, October 18, 1863, and sketched it in the diagram, fig. 3 (reproduced from memory; part of the luminous tail spreading

Fig. 3.

Athens, Oct. 18, 1863, 14 h. 55 m.   Course of the meteor E. to W.

eastward behind the meteor is left out for want of space). The atmospheric resistance acts more powerfully on bodies of comparatively greater surface, while the larger and heavier ones move forward. The most minute particles remain behind in form of a tail. The telescope has dissolved into a group what seemed to be a single meteor; it is as possible that bodies nearly approaching each other, and even wrapped up in the same meteoric envelope, may proceed in common. The ideal sketch, fig. 4, may

illustrate such a supposition. The largest fragment possesses, of course, the greatest amount of momentum; although pressed backward it still leads in the van, the smaller ones being likewise

Fig. 4.
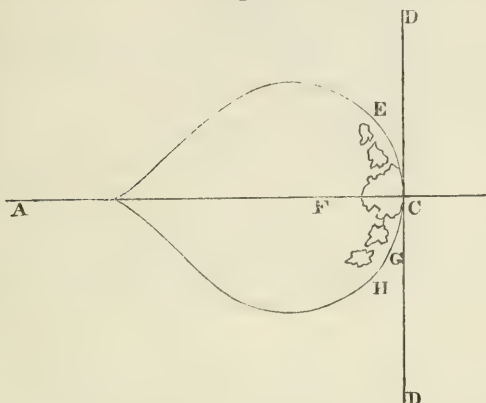
pressed backwards. Those next to the larger ones cannot totally remain behind, as the larger leading fragments have determined the elements of the igneous globe by producing a centre of elasticity (C) before it. All of them (E, F, G, H) must move forward within the anterior portion of the igneous globe, each single fragment in rotation on its own axis and around the axis of the line of course, and thus probably frequently come into mutual contact, by which their enamelled crust is damaged, as at Knyahynia, where nearly all the specimens collected were found more or less damaged. Of course the smaller fragments will follow the largest mass—although instances of several groups following each other have been frequently observed.

Ad 5. *The meteor, having reached the term of its cosmical course, becomes stationary.*

Ad 6. *The development of light and heat ceases; the surrounding air, suddenly rushing into the vacuum within the igneous globe, causes a detonation.*

The igneous globe becomes stationary the moment the action of the *vis viva* of the movement has reached its term. At the same moment its equivalent has occurred to our senses. We have perceived the last seconds of the movement, together with the great display of light and heat into which it has been transmuted. Electricity has not been mentioned, as its presence has not yet been ascertained by direct observation, although its action may be supposed. Lightning itself is but " a transmutation of electricity into light;" and the late Professor Plücker,

certainly an authority in physical questions, expressly asserts, with reference to the phenomena in Geissler's tubes, that *" there is no electrical light in the abstract sense of the term,"* and that *" the particles of the gaseous substances become incandescent."*

At the moment when the cosmical velocity, as well as light and heat, is exhausted, the tension of the vacuum within the igneous globe, entirely depending on this velocity, comes to an end, and the meteor obeys only the law of gravitation, causing it to fall on the earth's surface.

b. *Acoustic Phenomena.*—The sudden irruption of the surrounding atmosphere into the vacuum of the igneous globe causes a violent sound, in the same way as when gunpowder explodes, or whenever longitudinal oscillations take place.   One or more detonations are heard, sometimes at intervals, the last one being the most intense.   If previously masses, or groups of meteoric swarms, have separated from each other, repeated detonations may be expected.   At all events these phenomena still require repeated and close observation before general conclusions can be founded on them.   As we have seen above, Professor Daubrée (p. 16) declared it impossible for him to give a sufficient explanation of the detonations, and acknowledged the special difficulty offered by successive detonations, suggesting the idea of successive discharges and " recharges " of what he terms the " appareil explosif."   It is, indeed, impossible to explain a phenomenon having no real existence ; the subject of explanation can only be the " detonations " connected with the fall of meteorites that have reached the terrestrial atmosphere, the term of their cosmical career.

Ad 7. *Compensation of temperature takes place between the internal nucleus and the external surface.*

Without doubt the meteorites, before reaching the terrestrial atmosphere, take their course through a space of intensely cold atmosphere[*].   Professor Daubrée (p. 8) explains by this circumstance how the meteorites of Orgueil, containing substances easily alterable by heat, could reach the terrestrial surface without alteration of their chemical constitution.   The incandescence of the enamelled crust during its formation is likewise not to be doubted.   The efficient cause of this high temperature ceases as soon as the meteorite has become stationary.   During its subsequent fall to the surface time is left for a compensation between the melting temperature of its crust and the intense cold of its nucleus ; and generally fallen meteorites are warm, but not incandescent.   Some meteoric irons (as at Caritas Paso in Corrientes, 1844) have been found incandescent in their interior— an apparent exception, to be explained by the high thermal con-

[*] Von Haidinger in Vienna Acad. Proc. vol. xxxiv. p. 11, January 7, 1859.

ductive power of iron. In the report on the meteoric fall of Dhurmsala (India) some fragments are said to have been " intensely cold." Conceivable as this fact may be, meteorites having undergone the intense cold of cosmical space before their arrival in the earth's atmosphere, it is a new proof with what caution facts must be ascertained and asserted by future observers : in the first, superficial reports on the fall of Knyahynia in Hungary (July 9, 1866), the meteorites were said to have been as cold as ice ; further inquiries elicited that the few specimens collected immediately after the fall had been of no higher temperature than any other stones exposed to the sun for some time would be.

Ad 8. *The meteorite, now subject to the law of gravitation, falls on the surface, its temperature being in direct proportion to the thermo-conductive power of its substance.*—This proposition, being a necessary corollary of proposition 7, requires no special discussion.

The enamelled crust of meteorites offers some peculiarities, among which one is important enough to deserve special notice.

c. *Seams of enamelled crust.*—These seams are eminently conspicuous on the meteorites of Orgueil and of Stannern. Von Haidinger* admits that any meteoric fragment of somewhat regular figure, having entered the limit of the terrestrial atmosphere, persists in one and the same direction, determined by its figure, during the period whilst its enamelled crust is forming, and that the differences shown by this crust depend on the position of the surface-planes of the meteorite— the anterior plane being surrounded by a characteristic seam of enamelled crust, distinguishing it from the lateral and posterior planes. Professor Daubrée acknowledges also two forms of crust, which, as he says (p. 6), may serve to indicate the direction of the meteorite's course ; but he submits (p. 7) that " the meteorites showing both varieties of crust have undergone two successive superficial fusions during their atmospheric course —at first a general one of the whole substance, perhaps connected with the heat attending the explosion, then a greater degree of fusion on the portion pressing before it the air with an intensity proportional to the enormous velocity of movement." He supposes evidently an explosion to be the first cause of the formation of a crust ; while von Haidinger thinks the detonation indicative of the final moment, when the meteorite, with its crust completely formed, becoming stationary, and the emission of light having ceased, falls to the surface of the earth just as any other

* Acad. Vienna Proc. vol. xl. pp. 525–536, April 19, 1860, and vol. xliv. (2) pp. 790–795, May 16, 1862.

heavy body. M. Daubrée (p. 7) observes that "the air thus pressed away acts as a flame from a blowpipe on the anterior portion of each splinter," thus giving to understand that he considers the meteorite in question to be rather a fragment detached from a larger body than an individual object. Two consecutive formations of crust are hardly admissible within the brief period of some few seconds. The meteorites of more or less *discoid* form have given rise to a remarkable difference of opinions. Von Haidinger* concluded, from the position of the seams of enamelled crust on a discoidal fragment from Stannern, that the broadest plane rotating around the axis of movement must have necessarily been opposite to the resisting atmosphere. A portion of the *vis viva* of rectilineal movement is changed into rotatory movement in consequence of atmospheric resistance; and rotation may be increased so far as to produce *disruption,* as in the meteoric falls of Quenggouk in Pegu, December 27, 1857, according to Dr. T. Oldham's report †, and of Knyahynia, where the largest fragment had been dug out of a depth of 11 feet, evidently in a state of disruption. The large meteorite of Orgueil, figured by Professor Daubrée (plate 1. figs. 1 & 1 *bis*), is also characteristically discoidal, and is commented upon by the author (p. 9) in the following terms :—" This splinter, resembling a thick shell detached from a curved surface, instead of traversing the air in the direction of its breadth, as a flat stone thrust with violence would have done, has pursued its course with its large surface turned forward . . . . the fragment has been projected at the moment of the explosion with a celerity too great to allow it to change its initial position for one offering less resistance." These views may be applied to a heavy body (such as a stone) intentionally thrust by a thinking being, but not to a mass of inorganic substance acted on by a *vis viva* propelling it at the rate of four geographical miles per second until it is superseded by atmospheric resistance. The position with the broad surface forward is a necessary one. The supposition of a determined " explosion " (at least in the proper sense of the term) rests on insufficient ground. Professor Daubrée puts the question (p. 13) "whether a portion of the bolide's mass leaves the atmosphere after the explosion "—a question to be affirmed with some probability, according to his views, as he says (p. 15), " Consequently matters proceed as if the greater portion of the meteoric mass left the atmosphere to continue its cosmical course, leaving behind only some few particles whose velocity had been diminished in consequence of the explosion." This supposition is not applicable to the totality of the meteoric phe-

* Vienna Academy Meeting, May 15, 1862.
† Vienna Acad. Proc. vol. xliv. p. 637.

nomena hitherto observed. The fall of Knyahynia, July 9, 1866, may serve for an example. The meteor approached the terrestrial surface with only 6° zenithal distance, in a direction N. 76° 30′ E. to S. 76° 30′ W. It is certainly not to be supposed that, subsequently to the explosion, a portion of the meteor would have left the atmosphere under the same angle; nor is such a supposition admissible in the fall of Orgueil, as described by Professor Daubrée. The following data are taken from his communications to the Academy of Paris[*]: fig. 5 is the plan

Fig. 5.
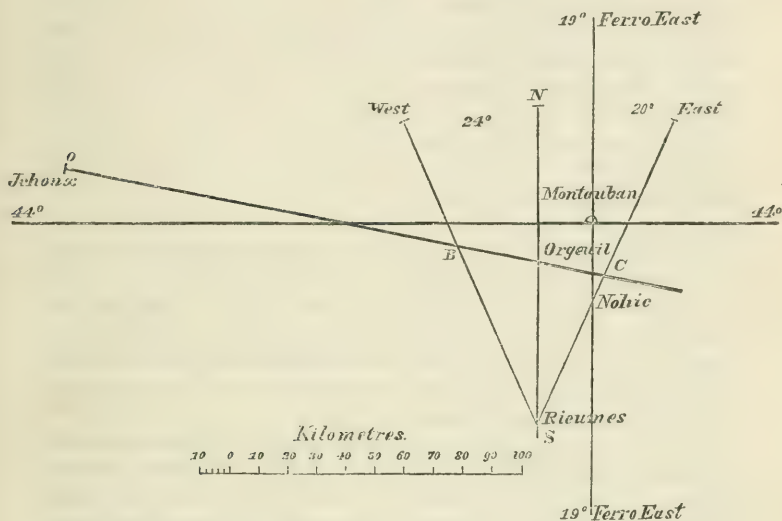
of the region within which the phenomenon was observed. M. Laussedat's report to Professor Daubrée gives the important fact that the meteor was seen falling down "vertically" at Ichoux (Département des Landes). The line B C in fig. 5 represents consequently the projection of the meteor's course on the surface of the earth. M. Lajous's report to M. Petit, dated from Rieumes (Haute Garonne), supplies the following data :—Beginning, azimuth N. 24° W., altitude 22°; final term, azimuth N. 20° E., altitude 16½°; and hence we have, in fig. 6, A B = 29·49 kiloms. (3·973 German geographical or 17·81 English miles), C D = 17·77 kiloms. (2·394 geogr. or 11·24 English miles). Light first appeared at an altitude of 29·49 kiloms. (3·973 geogr. or 17·18 English miles) ; and the phenomenon ended above Orgueil and Nohic at an altitude of 17·77 kiloms. (2·394 geogr. or 11·24 English miles). The fall

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[*] May 30, 1864, *Comptes Rendus Acad. Sc.* vol. lviii.

of the meteor in a distance of 58 kiloms. (7·815 geogr. or 36·68
English miles) amounted to 11·72 kiloms. (1·579 geogr. or 9·332
English miles).   The obliquity of the course towards the surface
is therefore $=11° 26'$, and the zenithal distance of the fall $=$
78° 34'.   The meteorites of Orgueil, their cosmical velocity
having been annihilated by atmospheric resistance, fell down as
any other heavy body would have done.   Had they pursued
their course to the last, they would have touched the ground
about 90 kiloms. (12·12 geographical or 56·92 English miles)
further off, at all events still W. of Montpellier.   Such a
course would be quite incon-
ceivable if supposed to have
been pursued by the greater
portion of the meteorite's nu-
cleus, after the extinction of
the light concomitant with
the transmutation of the *vis
viva* of movement.   M. Bagel
remarked that after extinc-
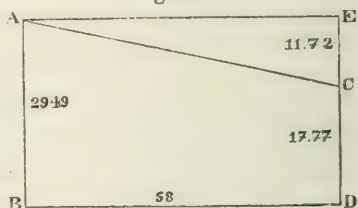
Fig. 6.

tion of the brightest light the meteor seemed to pursue its way
for a brief time, emitting a feeble reddish light : this may indeed
have taken place during the brief moment in which the in-
candescent crust fell to red-hot temperature.   A bird or any
creature organized for flight and endowed with volition may
change the direction of its movement at its own choice ; but this
is not the case with an inorganic body.

    6. B. *Primordial formation of Meteorites.*—It must be ad-
mitted as a fundamental principle that, from the first moment
of their existence, the material elements possessed no other
properties, and the *vis viva* acted according to no other laws,
than those which we know to exist at present.   The first
consequence of creation (an act quite above human comprehen-
sion) must therefore have been aggregations of pulverulent sub-
stances spread about in the intensely cold cosmical space.   The
successive phases of formation may be supposed thus :—

    (1) Creation, matter *in statu nascenti.*
    (2) Cosmic bodies in the state of aggregations of pulverulent
matter.
    (3) Increase of temperature by pressure of the superficial
strata against the deeper ones, and by natural pressure of homo-
geneous and heterogeneous particles, the interior of the cosmical
body against its superficial crust*.
    (4) Formation of a solid crust, the innermost space still pro-
gressing in solidification.
    (5) Explosion of the cosmical body in consequence of the dif-

            * Humboldt, *Cosmos*, vol. i. p. 209.

ference of internal and external expansive tension ; the fragments thrown out in every direction pursue their course through cosmical space.

The effective formation of the mass of meteorites falls entirely within the third period ; it can, however, only proceed very gradually. At the moment of a destructive catastrophe many substances, different in nature or in progress of evolution, may coexist, all of them, however, impressed with a common character. Soft or hard meteorites, non-crystalline and more or less distinctly crystallized ones, bear the mark of more or less advanced evolution ; all of them, however, show a distinct *tufaceous* structure and a *breccia-like* formation, the meteoric iron, partly eminently crystalline, representing veins included in a more solid matrix. Traces of *gradual* formation occur everywhere ; nor do the meteorites of Orgueil prove an exception to this rule. Although composed of substances of rarer occurrence, they show the general tufaceous type. Professor Daubrée says (p. 3), " They [the common meteorites] seem indeed to have been formed at once ; it seems this was not the case with the mass of which the Orgueil meteorites are fragments." And, speaking of the common meteorites, he says (p. 4), " whose general character is to be capable of being produced *viâ siccâ* under the action of high temperature." Certainly the experiments of the distinguished author concerning artificial production of meteoroidal substances have thrown considerable light on their mode of formation ; it must, however, be stated that the more crystallized these bodies are, the more remote they stand in the scale of evolution from the primordial pulverulent condition of their substance. It cannot be doubted that the varieties of meteorites, between dust and highly crystalline iron and stone-like substances, indicate distinct terms of evolution, the pulverulent agglomerates in the intensely cold cosmical space being heated by progressing solidification and pressure to the end of their existence as cosmical bodies. The simultaneous presence of heterogeneous particles within a body of notable dimensions may well be supposed*.

The lines, in the interior of meteorites, identical in composition with the enamelled crust, although of inferior formation, deserve some attention. Their formation probably coincides with the consecutive and far advanced process of solidification. As in telluric rocks, progressive evolution and mobility of particles, even of solid substances, certainly takes place in the interior of meteoric bodies, although not assisted by the dissolving power of water. In places where pressure and temperature come to their maximum, partial fusion

* See Vienna Acad. Proc. vol. xliii. (1861) pp. 489–526.

may begin, at first where the larger separate portions of the whole are pressing on each other. Vein-like strata, occasionally with sliding laminæ of iron (the first beginning of iron-veins), form. In another paper* Professor Daubrée insists again on the possibility of a meteoric body, after the fall of a comparatively minute portion of its substance, continuing its course from the terrestrial atmosphere into cosmical space. The altitude of the Orgueil meteor at the moment it entered the atmosphere is said (*loc. cit.*) to have been about 65 kiloms.; while according to the geographical distance, communicated by M. Lajous and published by Professor Daubrée (see above, p. 255), it cannot have exceeded 29·49 kiloms. Professor Daubrée says, further (p. 2), that the fall of meteorites is immediately preceded by their "nearly horizontal" course. In fact the inclination towards the horizon was observed to be about 11° 26' for the meteorite of Orgueil, 44° (according to Professor Galle) for that of Pultusk, and 84° for that of Knyahynia (according to von Haidinger).

7. *The Professors of Casale.*—MM. Goiran, Zannetti, Bertolio, and Musso, in their report on the fall of Villanova and Motta dei Conti†, admit, as an undoubted fact, the fall " of a whole group " of meteorites (p. 33). In a further passage ‡ the existence of a single body, broken into larger and smaller fragments by " one or more explosions attended with a detonation," is asserted. This explosion is explained, without any further proof, by admitting an " elevation of temperature, imparting to the large proportion of gaseous substances included in these bodies an amount of elastic power sufficient to produce an explosion " (p. 66). The authors § admit the entrance of a " group " of meteorites into the terrestrial atmosphere, as the formation of a complete crust could not be accounted for by any other supposition.

8. *M. Leymerie.*—In a letter dated from Toulouse, June 10, communicated by Professor Daubrée to the Academy of Paris in the Meeting of May 30, 1864, M. Leymerie says, " Allow me to insist on the circumstance that all the separate pieces (ten in number) which I had occasion to see offered a distinct form and were wholly covered with ' vernice ' (enamelled crust). It is evident to me that all these pieces, agglomerated closely to each other, formed but one whole, a ' swarm,' to use M. Haidinger's term. After the explosion they were separated and dispersed, no fracture having taken place."

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* *Nouvelles Archives du Muséum*, tom. iii. p. 1 (1866).

† *Bolletino dell' Osservatorio Meteorol. di Moncalieri*, vol. iii. Nos. 3, 4, 6, and 8, Suppl.

‡ No. 7, p. 65.   § No. 8, Suppl. p. 90.

9. Professor J. G. Galle.—This distinguished astronomer has published a paper on the meteoric fall of Pultusk, January 30, 1868, in the Transactions of the Silesian Society for 1868. The views of the celebrated Director of the Breslau Observatory are, in many particulars, concordant with those of von Haidinger, as he says (p. 28), "The mode of expression generally used, by which the division of a meteor at the moment of extinction is designated by the terms ' explosion ' or ' diffraction,' is, for reasons subsequently to be discussed, improper and inadequate to the real fact. We can only admit a *dispersion of the meteoric particles which have arrived as separate bodies at the point of extinction.* This supposition alone admits an explanation of the *acoustic* phenomena and of their peculiar series, beginning with one or more chief detonations." The meteorites of Pultusk fell *nearly vertically from the point of dispersion.* The inclination of their cosmical course to the horizon was 44°. Compared with fig. 6, the altitude of the entrance into the atmosphere A B, the altitude at the term of the cosmical course C D (C being the point of dispersion), the distance B D, the inclination of the cosmical course C A E, stand in the following proportion :—

		A B.	C D.	B D.	C A E.
Pultusk	. .	40	$5\frac{1}{2}$	25	44°
Orgueil	. .	3·975	2·394	7·815	11° 26′

Geographical miles.

Professor Galle says about the *origin* of meteorites, " What may have been the origin of such a meteoritic swarm entering the solar system with a velocity of one to two miles per second, is a problem still more difficult to solve than the origin of the periodical swarms of falling stars, whose connexion with the comets has been ascertained by Professor Schiaparelli's brilliant discovery, and which, on account of their nearly parabolical orbit, may be considered to be cosmical clouds, endowed with a small power of movement, and coming at some point or other within the solar sphere of attraction." And as to their velocity, " It is at least one or two miles per second less than the average velocity of falling stars, as admitted by Professor Schiaparelli ; the orbit through the solar system is therefore an *hyperbola.*"

10. Dr. G. vom Rath has published a notice concerning the fall of Pultusk, especially with regard to the chemical and mineralogical constitution of the meteorites, of which he had occasion to examine above 1200 single specimens, and to compare them with those preserved in the museums of Bonn and Berlin. Fig. 1 *a, b, c* of this notice reproduces the anterior surface of a meteorite weighing 870 grms., on which the author (p. 4) com-

ments thus : " The anterior portion of our meteorite is characterized beyond any doubt by the presence and the direction of the seams of enamelled crust, by means of which the position of the stone during its telluric course before its fall may be ascertained." According to Dr. vom Rath (p. 6) the meteorites of Pultusk cannot be considered to be the results of the diffraction of one single cosmical body, but must rather be supposed to be a *swarm* of larger or smaller planetary individuals that had come within the sphere of terrestrial attraction and having most of them undergone diffraction, although the strongly flattened cosmical figure could still be traced on some of them. As von Haidinger remarks, seams of enamelled crust could not be formed on meteorites of quite irregular form, whose position necessarily changes at every moment of their course. During its cosmical course a swarm could remain in repose ; but as soon as it enters the terrestrial atmosphere, the individuals composing it, in collision with each other, must undergo loss of substance. During the continuation of the cosmical course through the atmosphere, the conditions for incrustation constantly remained extant, and fragments of secondary origin could be incrusted again. From the moment of detonation the incrusted meteorites, having reached their point of dispersion, fall down individually in more or less vertical direction ; they may thus damage each other, but no new incrustations take place. Figs. 7 *a* and 7 *b* in Dr. vom Rath's paper (p. 9) deserve particular attention, representing a completely incrusted meteorite with an aggregation of thirty to forty minute fragments which had evidently adhered to it during its course when its velocity began to suffer diminution. An analogous circumstance has been observed by von Haidinger on a meteorite from Stannern ; it is, however, characteristic of those from Pultusk. Dr. vom Rath says, further, (p. 27), " The meteorites, not quite dissimilar to telluric rocks, although for the most part so different from them, indicate conditions of formation as they were never found united within the known portion of the terrestrial crust ;" and (p. 12), " The explanation of the surfaces clothed with metallic or sulphuretted iron (*metallische Harnische*) presents great difficulties ; at all events, they are of *cosmical* origin, while the lines and surfaces of fusion are of telluric origin." This last assertion appears to von Haidinger not to be quite unobjectionable. The enamelled crust, formed during the course, may, under peculiar circumstances, penetrate here and there into the substance of the meteorite ; it must, however, be admitted that the internal clefts and fissures, partly filled up with a black enamel-like substance, have originated during the cosmical period of the mass, before its being broken into pieces which immediately began their course through

cosmical space. A specimen from Pultusk in the Imperial Mu-
seum of Vienna shows, in a breadth of about 2 inches, seven
planes of separation filled up with enamel-like substance*. M.
von Fritsch has published a valuable notice of the chemical
constitution of the Pultusk meteorites in No. 5, pp. 92–94 of
the Proceedings of the Imperial Geological Institute for 1868.

11. Professor G. V. Schiaparelli.—This illustrious astronomer
of Milan has published (1867) "Notes and Reflections on the
Astronomical Theory of Falling Stars" in the *Memorie di Ma-
tematica e Fisica della Società Italiana delle Scienze*, ser. 3,
vol. i. part 1, p. 153. The author treats only collaterally of
the formation of meteorites and the phenomena connected with
their fall, his chief purpose being the development of his re-
markable discovery concerning the concordance of the course
of meteoric currents with the orbits of some comets. This
subject, merely alluded to with great reserve by that noble pa-
triarch of science, Alex. von Humboldt, in his 'Cosmos' (vol. i.
p. 142), could only be discussed by way of numerical com-
parison subsequent to the preparatory investigations of MM.
Leverrier and H. A. Newton. Professor Schiaparelli (p. 198)
admits the transmutation of the *vis viva* into heat and light, and
alludes (p. 255) to the probability of the meteoric iron having
at first been gaseous, as the existence of crystals of olivine in
the interior of the "Pallasian iron" could not be explained
by means of any other supposition. Alexander von Humboldt†
declares inadmissible, even in the regions of mere probability,
any unproved arbitrary opinion; and, indeed, those who have
specially studied the substances composing the solid crust of our
globe will explain the fact here in question rather by the mobi-
lity of the minutest particles of even solid substances in the
course of their successive development than by ascribing to matter
such proprieties as they are not known to possess at present.

12. Dr. E. Weiss begins his paper "On Falling Stars"‡ with
the words, "It can no longer be doubted that the igneous phe-
nomena connected with falling stars owe their existence merely
to the resistance of the atmosphere." He assumes that every year
the earth gives to innumerable meteors a direction which takes
them out of the solar system into the region of the fixed stars.
The contrary process may likewise take place, as Professor Galle
(see above) has proved the meteoric swarm of Pultusk to have en-
tered the solar system with a velocity of at least one or two geogra-
phical miles per second. Dr. E. Weiss supposes the nuclei of the
falling-star meteors to be small, without determining their size;

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but, indeed, applied to cosmical bodies, the conception of magnitude will always be relative. He concludes, further, from the more intense emission of light in higher atmospherical strata, that such meteors have a greater amount of velocity than those appearing with less splendour in the lower regions of the atmosphere. Director Julius Schmidt came to the same conclusion from his observations of the meteors in the course of 1851*. These observations induced Haidinger† to suppose a connexion between the accelerated formation of the luminous envelope in the progress of motion and the tenuity of particles of agglomerations of cosmical pulverulent substance, in conformity with the views expressed in Mr. A. S. Herschel's letter on the nature of planetary corpuscles, addressed to Abbé Moigno‡. Dr. E. Weiss has still some doubts about the explanations of the prevalence of falling stars or of meteors, according as the meteoric bodies and the terrestrial globe are moving in the same or in opposite directions.

13. Messrs. R. P. Greg and A. S. Herschel published in 1867 an 'Atlas of Charts of the Meteor-tracks contained in the British-Association Catalogue of Observations of Luminous Meteors, extending over the years 1845 to 1866.' This Atlas comprehends twenty-two celestial charts of Bede (three for January, August, and September, two for February, April, October, and November, and one for each of the five other months), on which are entered the dates of the phenomena, the limits of their duration, the position of the points of radiation, their denomination as proposed by MM. Heis and Greg, and their peculiar characters with regard to all general and special meteoric currents hitherto observed in the northern hemisphere. An appendix contains a synoptic Table of the points of radiation in the northern hemisphere from January 1 to June 30, and from July 1 to December 31, and another, for each day of the year, of the radiations of fifty-one epochs of meteoric currents, as determined by Professor Heis. A paper published in the 'Scientific Review,' London, June 1, 1868, has added twelve new points of radiation of meteoric currents to the sixty points already known. Mr. Greg has undertaken the laborious task of reducing above one thousand new observations made by M. Jezioli at Bergamo. An enormous amount of observations is also treasured and systematically arranged in Messrs. J. Glaisher, R. P. Greg, E. W. Brayley, and A. S. Herschel's "Reports of Observations of Luminous Meteors," addressed to the British Association for the Advancement of Science§. Mr. A. Herschel's spectrum-

* See Vienna Acad. Proc. vol. xxxvii. pp. 803–820.
† Vienna Acad. Proc. vol. xlix. sect. 2, pp. 6–61, January 8, 1864.
‡ See *Les Mondes*, 1ᵉ année, tom. ii. liv. 14, November 5, 1863.
§ See Report of Brit. Assoc. 1866 & 1867.

observations of several meteors, and his discovery of the spectrum-line of *sodium* as distinct as in the spectrum from a gas-flame of a Bunsen's burner or of a monochromatic flame richly fed with common salt, together with his observations on the forms and modifications of the luminous remains of meteors, deserve special attention, as they seem to confirm the supposition of a gaseous envelope forming around the progressing nucleus. Mr. Herschel says*, " Each new acquisition of knowledge, however unforeseen may be its origin, tends to support the theory of Chladni, and to confirm the belief that shower-meteors and shooting-stars are actually aërolites of small dimensions. In whatever manner aërolites and shooting-stars are related to each other in their astronomical and other peculiarities, they will evidently require a vast number of future experiments to unfold their real source." The meteor telescopically observed by Mr. H. Christ at Basle, June 11, 1867, 8.30 to 9 P.M., and pursued in its further course, N.W. to S.E., by Professor E. Hagenbach-Bischof at Basle and by M. de Fonvielle at Paris, excited likewise a great deal of interest.

14. Professor J. J. Omalius d'Halloy, in the eighth edition of his *Précis élémentaire de Géologie*, has devoted a special chapter to meteorites and the phenomena attending their apparition, in which he briefly expounds von Haidinger's views on the subject. He says (p. 118), "As to the light manifested by bolides in their course through the atmosphere, and the vitrification undergone by their crust, M. Haidinger explains both these circumstances by the development of heat and light resulting from the compression of the air in consequence of the rapidity of the meteors falling towards the terrestrial surface; and he ascribes the detonations attending their fall to the irruption of the ambient air rushing into the vacuum left by the passage of the bolide—that is, of the solid body and the portion of compressed air around it."

15. *Conclusion.*—The facts at present ascertained, together with the conclusions derived from them, allow us to distinguish four consecutive periods in the existence of meteorites : (1) primordial formation; (2) progressive movement through cosmical space; (3) fall to the terrestrial surface; (4) the fallen substances themselves now become subjects of scientific investigation. They are at once subjects of investigation and meditation for the mineralogist, the chemist, the physicist, the meteorologist, the geologist, the geographer, and the astronomer. The English translation of von Haidinger's " Considerations on the Phenomena attending the Fall of Meteorites on the Earth," read before the Imperial Academy of Vienna, March 14, 1861, was published

* Report to the Brit. Assoc. 1866, p. 146.

in the November and December Numbers of the Philosophical
Magazine for 1861.   Other notices by the same author were
communicated and commented upon by Mr. R. P. Greg at the
Manchester Meeting of the British Association.   Meantime the
Imperial Museum had received specimens of the Tula iron from
Dr. Auerbach, and a rich collection of East-Indian meteorites
through Dr. Thomas Oldham's kind intervention; and Messrs.
Laurence Smith, B. V. Marsh, H. A. Newton, Baron Reichen-
bach, and others had published their theoretical views on me-
teorites. Dr. Buchner had lectured on the same subject at the
thirty-sixth Meeting of German Naturalists at Spire, 1861.
Another paper by von Haidinger had been communicated to the
Academy of Paris by M. Elie de Beaumont*.   The collections
of meteorites have been greatly augmented since then. The Mu-
seum of Munich numbers (March 1, 1868) twenty-two localities
of iron or of lithoid meteorites; the University Museum of Göt-
tingen (July 1, 1868) 176 localities, 99 of lithoid meteorites, 77
of meteoric iron; the Museum of the Geological Survey of India
in Calcutta (December 1867) 254 localities, 159 of lithoid me-
teorites, 95 of meteoric iron.

XXXV. *Contributions to the Mineralogy of Nova Scotia.   By*
Professor How, *D.C.L., University of King's College, Wind-
sor, N.S.*

[Continued from vol. xxxv. p. 41.]

IV. *Lignite—Semibituminous Coal—Cannel Coal—Turgite—
Delessite—Fahlunite—Silicoborocalcite.*

*L*IGNITE.—In the eastern part of the province there are
found in the carboniferous districts, but apart from the
beds of coal, sulphides of metals associated with coaly matter
having the characters of lignite. The sulphides are relatively
large in quantity, and frequently consist of iron and copper py-
rites, with a very rich sulphide of copper, apparently vitreous,
though called grey. The enclosing rock of such specimens of
these copper ores as I have seen is sandstone, and it is generally
more or less impregnated with green carbonate.   In one instance
galena occurs with iron pyrites in a conglomerate of limestone
and siliceous pebbles.   The masses composed of the carbonaceous
and metallic minerals generally exhibit the form of branches and
trunks of trees; and Dr. Dawson points out† that those con-
taining copper result from the action of vegetable matter on
waters containing sulphate of the metal.   The formation of

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* See *Comptes Rendus,* vol. liii. (1861) pp. 456–461.
† Acadian Geology, p. 327.

galena would involve some secondary action. The deposits are not thought to be of mining-importance, except in two instances, in which the ores of copper are considered to exist in promising quantities: in one of these, indeed, lately discovered there is said to be a bed some feet thick; a sample of the ore sent me gave 29·5 per cent. metallic copper.

I have not been able to find more than one complete ultimate analysis of a lignite; this is one of the fourteen analyses given by Dana*. In a minority only of the others is the existence of nitrogen mentioned; the quantity is given in no case but that referred to, the analysis of Vaux, who found 0·57 per cent. From this result and the following ultimate analysis of a lignite from one of the deposits above mentioned, for which I am indebted to Professor Anderson of Glasgow, it appears that the quantity of nitrogen is somewhat less in this mineral than in black coal.

The lignite selected for examination occurs in the carboniferous district of Pictou county, with copper ore and common iron pyrites. It is nearly black, and retains on some faces a fine-grained woody structure, not very obvious to the naked eye, but distinct under a glass: these surfaces are dull; those at right or oblique angles are black, of almost resinous lustre, and without structure: it is sectile, and easily broken into angular fragments which receive polish under the burnisher. The mineral evidently belongs to the variety of brown coal called jet. Boiled in potash, it scarcely colours the fluid. Ignited, it gives a transient feeble yellow flame, and afterwards glows for a considerable time, evolving the greasy odour often observed with lignite; in a closed tube the product is chiefly water with a little yellowish matter; the vapours have an alkaline reaction. Ultimate analysis gave :—

Carbon	74·5
Hydrogen	4·3
Nitrogen	1·0
Oxygen	18·7
Ash	1·5
	100·0

It does not appear that any inquiry has been made as to whether lignites vary in composition according to their geological age. As the circumstances under which they were formed must have been analogous, the only differences to be expected at the corresponding stages of conversion are those depending on variations in the original material. Hence nothing like strict comparison can be made; the following, however, is not without interest. The large deposits of brown coal found in Germany

* Mineralogy, 5th edit. p. 758.

are of tertiary age; and Liebig has shown* that there is a nota-
ble difference among these in chemical composition. The more
complete the obliteration of the woody structure, the greater the
departure from the relative proportions of the organic elements
in the original material. The wood-coal from Ringkuhl, near
Cassel, is seldom found with the structure of wood, and is there-
fore more comparable with the Pictou lignite than any in which
this is retained; its composition is given as—

$$
\begin{array}{lr}
\text{Carbon} & 63\text{·}83 \\
\text{Hydrogen} & 4\text{·}80 \\
\text{Oxygen} & 25\text{·}51 \\
\text{Ash} & 5\text{·}86 \\
\hline
& 100\text{·}00
\end{array}
$$

and the approximate formula (old notation)

$$C^{32} H^{15} O^{9}$$
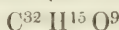
is added as deduced from these numbers: for comparison' sake I
have calculated a corresponding approximate formula from the
analysis of the Pictou lignite; it is

$$C^{32} H^{12} O^{8}.$$

Hence, as compared with wood, for which Liebig gives the
formula

$$C^{36} H^{22} O^{12},$$

there is a further loss of one atom of water and two atoms of
hydrogen, marking the difference between the carboniferous and
the tertiary lignites in question.

As I have before pointed out†, the true ratio of carbon to
hydrogen, in such minerals as contain these elements together
with oxygen, is only brought out after deducting the amount of
hydrogen equal to that of the latter element present. In the
case of the two lignites just spoken of, the ratio stands thus:—

	Pictou.	Ringkuhl.
Ratio of C to H without deducting $H = O$ present	$100 : 5\text{·}77$	$100 : 7\text{·}52$
Ratio of C to H after deducting $H = O$ present	$100 : 2\text{·}63$	$100 : 2\text{·}50$

The effect of this deduction is obvious enough; at the same
time a very close accordance is exhibited in the ratio between the
remaining elements. How much this differs from that existing
in bituminous coals is seen by the following comparisons. While
numerous coals of the province have been submitted to prismatic

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* Agricultural Chemistry, 4th American edit. p. 368 *et seq.*
† Mineralogy of Nova Scotia, p. 25.

analysis*, one only has had its ultimate composition determined, that, namely, found at Springhill, Cumberland county. All the true coals of the province are bituminous, and they would, no doubt, give results comparable with those from the Springhill coal below and the bituminous coals now selected as representatives from among those analyzed by myself during the British Admiralty Coal-Inquiry†.

Bituminous coals.		Percentages.			Ratio of carbon to hydrogen.		Authority.
		Car-bon.	Hydro-gen.	Oxy-gen.	By ana-lysis.	On deduct-ing H = O.	
Nova-Scotian.	Springhill. Cumb. co.	72·00	5·02	7·26	100 : 6·97	100 : 5·72	Woodhouse & Jeffcocke
Welsh.	Duffryn	88·26	4·66	0·60	100 : 5·28	100 : 5·18	H. How.
	Newydd	84·71	5·76	3·52	100 : 6·79	100 : 6·28	,,
	Ebbw Vale ...	89·78	5·15	0·39	100 : 5·73	100 : 5·67	,,
Scotch.	Grangemouth...	79·85	5·28	8·58	100 : 6·61	100 : 5·27	,,
	Fordel	79·58	5·50	8·38	100 : 6·93	100 : 5·73	,,
English.	Broom Hill ...	81·70	6·17	4·37	100 : 7·55	100 : 6·88	,,
	Lydney	73·52	5·69	6 48	100 : 7·73	100 : 6·63	,,

Semibituminous Coal.—On the line of railway between Truro and Pictou, about twenty miles from the former town, a curious "vein" is found between slaty walls, consisting of black lustrous mineral formed into a sort of network by the intersection of numerous cross veins of fibrous ferriferous carbonate of lime, the whole being two or three inches thick. The black mineral looks something like graphite, is rather hard, H. = 2·5, and very brittle; ignited, it gives a luminous white flame, exfoliates, smells strongly of sulphurous acid, and burns with some difficulty to a reddish ash. In a closed tube it does not melt, scarcely changes its form, gives but a small quantity of smoke and oil; the fumes are very acid. For the carbon- and hydrogen-determinations which follow I am indebted to Professor Anderson. Ultimate analysis gave :—

$$\begin{array}{lr}
\text{Carbon} & 85\cdot33 \\
\text{Hydrogen} & 4\cdot99 \\
\text{Sulphur, nitrogen, and oxygen} & 4\cdot70 \\
\text{Ash} & 4\cdot98 \\
\hline
& 100\cdot00
\end{array}$$

Proximate analysis afforded me :—

$$\begin{array}{lr}
\text{Volatile matters} & 10\cdot90 \\
\text{Residue, or "coke"} & 89\cdot10 \\
\hline
& 100\cdot00
\end{array}$$

* *Loc. cit.* p. 33, and Dawson's 'Acadian Geology.'
† First Parliamentary Report by Delabeche and Playfair.

The residue, or "coke," consisted of a powder unchanged in appearance from that put into the crucible.

The foregoing results indicate a semibituminous coal: the mineral differs widely from the bedded coals of the province, which, as before mentioned, are bituminous; some, indeed, are so in rather a high degree. The mode of occurrence of this coal (the only one I have met with here approaching anthracite) is curious, and worth further investigation.

Cannel Coal.—A specimen "from an 18-inch seam at Little Glace Bay, Cape Breton," had the appearance of cannel coal, gave a brown powder and a brownish-black streak, burned alone when well heated in a flame. in a closed tube gave much volatile matter, and left a rounded swollen coke. Proximate analysis gave me :—

<div align="center">

Moisture	0·83
Volatile combustible matter .	30·07
Fixed carbon	44·42
Ash	24·68
	100·00

</div>

The amount of ash here, though considerable, is smaller than that in the well-known Scotch cannel from Capeldrae, which gives, according to Fyfe, 25·40 per cent. The volatile combustible matter is evidently high enough, in proportion to the fixed carbon, to mark the class of minerals to which the specimen belongs. This is the first example of Nova Scotian cannel coal yet examined, unless, as would be right according to some authorities, now perhaps diminishing in number, the remarkably rich oil-coal which I have called stellarite * is made to belong to the class.

Turgite.—This well-defined species may be recorded among the minerals of iron met with in the province. It occurs with brown hæmatite at Terry Cope, and at another locality, probably in the same county of Hants, according to my own examination of specimens sent me, and it will doubtless be found frequently elsewhere in the same association. Both the specimens in question afforded red powder, gave water on ignition, and decrepitated violently in the forceps before the blowpipe. The mineral from the last-mentioned locality was mixed with siliceous and calcareous matters; when these were deducted, the constituents of the air-dried substance were :—

<div align="center">

Water	5·51
Peroxide of iron . . .	94·49
	100·00

</div>

The formula of turgite, $2 Fe^2 O^3 + HO$, requires 5·32 per

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* Mineralogy of Nova Scotia, p. 24.

cent. water; hence there can be no doubt as to the nature of the species.

Delessite?—There are frequently seen in the amygdaloidal traps of the western part of the province masses of a soft, dark green, chlorite-like mineral; or perhaps there are more species than one having these characters, sometimes filling rock-cavities more or less completely, and occasionally coating or partly composing zeolites, as, for example, analcime, which in different localities has its colour thus made to vary through all shades to dark green. At one place, Two Islands, in the Basin of Minas, I observed, several years ago, large crystals of this species lying loose in a cavity of the trap above high-water mark, like specimens on the shelf of a cabinet. I could not get as many of these as I wished, nor examine the nature of the cavity, on account of the rapidly rising tide. Some of the crystals were colourless; one was most curiously composed: nearly half of the upper portion consisted of white partly transparent analcime; the rest, including the whole base, was almost entirely made up of a soft dark-green mineral like chlorite. The crystal had the form of analcime; it was about an inch and a quarter in diameter. I made no analysis of the green mineral in this case; but I examined a specimen of similar soft, green, greasy-looking mineral from cavities of trap on the Bay of Fundy. It was very dark in colour, opaque to the naked eye, translucent under the microscope, very soft, fusible on the edges to a black glassy bead before the blowpipe. It dissolved partially in slightly warmed hydrochloric acid, to which it gave both protoxide and peroxide of iron in large quantity. Water was lost on ignition and was so estimated; the amount obtained in this way, however, would be rather below the truth, owing to the absorption of oxygen by the protoxide of iron. Alkalies were not sought for. Analysis of the air-dried mineral gave:—

Silica	40·53
Metallic iron	12·50
Alumina	6·95
Lime	0·52
Magnesia	14·15
Gangue	2·87
Water (by ignition)	13·10
Oxygen and loss	9·38
	100·00

These results appear to place the mineral between Delessite and epichlorite, which have the following composition* :—

* Dana's 'Mineralogy,' 5th edit. pp. 493, 497.

	Epichlorite.	Delessite from Mielen.
Silica	40·88	31·07
Alumina	10·96	15·47
Peroxide of iron .	8·72	17·54
Protoxide of iron .	8·96	4·07
Magnesia . . .	20·00	19·14
Lime	0·68	0·46
Water	10·18	11·55
	100·38	99·30

As epichlorite occurs in veins in serpentine, and Delessite is found coating and filling the cavities of amygdaloid, the mineral now described is probably the latter, perhaps mixed with some free silica.

Fahlunite.—On the road between Windsor and Chester granite is found, at some point not mentioned to me, containing crystals which present the characters of chlorophyllite, or one of the varieties of Fahlunite. They are many- (perhaps 12-)sided prisms, sometimes half an inch across, greenish brown and dull externally; the distinct basal cleavage shows faces of a grey colour and vitreous lustre; weathered surfaces of this kind have the appearance of mica; hardness of the fresh faces about 3; streak grey and earthy. The mineral fuses before the blowpipe on the edges to a black slag, yields a little water on ignition, gives abundance of protoxide of iron to cold hydrochloric acid on standing for some time, and a very small quantity of peroxide; by fusion with alkaline carbonate its other constituents are found to be silica, alumina, manganese, a trace of lime, and a little magnesia. I made no quantitative analysis; but it is evident the mineral belongs to the species Fahlunite, which results from the alteration of iolite*.

Silicoborocalcite in a new locality.—It is an interesting fact that this new mineral, which was first described in the last part of these " Contributions"†, and which Dana has since called Howlite‡, has recently been found in gypsum in a new locality some thirty or forty miles N.E. of Brookville near Windsor, whence it was first obtained. The quantity is said to be by no means inconsiderable; and as I have shown§ that the mineral will probably admit of a special application, just as it occurs, in enamelling iron, it may prove to be a valuable addition to the already large and varied mineral resources of the province. The specimen by which I identified the species recently found was in

* Dana's ' Mineralogy,' 5th edit. p. 484.
† Phil. Mag. Jan. 1868.
‡ Mineralogy, 5th edit. p. 598.
§ Mineralogy of Nova Scotia, p. 141.

the powdery form described in the paper referred to above.   The mineral is reported to be most abundant in a " black band " of gypsum, and to be met with in nodules which are sometimes very much larger than any seen by myself near Windsor ; a good deal is said to have been thrown away by the quarrymen as being of no value compared with the plaster ; this is worth about 90 cents a ton, shipped, while the borate would probably realize nearly as many dollars, or, roughly, be a hundred times as valuable.   It was mentioned in the paper already twice alluded to, that, from what I had observed, the lowest beds of gypsum would probably afford the largest quantity of this mineral and its fellow borate : the new locality, as I understand, is one of those where the lowest members of the carboniferous series containing beds of gypsum are found.

XXXVI. *On the Galvanic Resistance of Liquids.* By Dr. Paalzow[*].

THE first trustworthy values for the galvanic resistance of a liquid (that of sulphate of zinc) are contained in a paper by Beetz[†].   The paper also contains the reasons for placing no confidence in previous statements obtained by other methods. It is only by the combination of zinc with sulphate of zinc, which Beetz made use of, that it is possible to avoid polarization and other changes of the electrodes as well as of the adjacent electrolytes.

I proposed to determine the resistance of other liquids as well as of sulphate of zinc, retaining, however, unpolarizable electrodes and their advantages.

In order more clearly to detect a possible influence of the chemical nature of the liquid on the resistance, I made the solutions of the acids and salts under investigation in chemical equivalents ; for it may be reasonably supposed that such an influence exists, as the conduction of electricity in liquids is always accompanied by chemical decomposition.

I then also examined the resistances of mixtures of saline and acid solutions in water with equal equivalents of water, in order to decide whether the current divides in accordance with Ohm's law.

In order, moreover, to ascertain whether there is with electrolytes, as with metals, a connexion between the conduction for heat and for electricity, I investigated the conducting-powers for heat of certain liquids whose galvanic resistances were known.

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[*] Translated from the Berlin *Monatsbericht*, July 1868.
[†] Poggendorff's *Annalen*, vol. cxvii. p. 1.

The fundamental idea which regulated the construction of the apparatus was the following. In order to limit a defined quantity of the liquid investigated, a siphon tube had to be filled with it, and this connected with the electrodes and the conducting liquids, so that the values for the resistance of the liquid in the siphon tube were as great as possible in comparison with the resistance of the conducting liquids and the inequality and polarization of the electrodes. But to exclude these latter unavoidable magnitudes in each individual experiment, a second siphon tube had to be connected with the electrodes in the same manner, the dimensions of this second tube being either considerably greater or smaller than those of the first tube. The resistance of the liquid alone could be determined from the difference of the two numerical values, provided that in the time which elapsed between two observations no alteration had occurred in the electrodes or in the conducting liquids. The resistances had to be referred to mercury as unit.

Two wide glass vessels were filled with a concentrated solution of sulphate of zinc, and as large electrodes as possible of amalgamated zinc placed in them. Over these electrodes porous cells were placed, which were connected by a siphon tube. Both cells and siphon tube were filled with the liquid to be investigated.

The zinc electrodes of the apparatus were connected with the conducting-wire of one branch of a Wheatstone's bridge, while in the other bridge a normal resistance-coil of 0·1 to 50000 Siemens units was introduced. Such a proportion of this resistance was introduced that the terminal of the wire of the bridge was as near the middle as possible.

When the observation for the first siphon tube was ended, the first siphon was replaced by a second, and, for control, frequently by a third. Beginning with the largest, the second was filled with the liquid contained in the first, the third with that in the second; so that almost exactly the same quantity of liquid was retained. After inserting suitable resistances, the numbers were read off on the measuring-line.

By preliminary experiments the resistance of the mercury was measured which filled the siphon tubes, and the difference of the mercurial resistances of every two tubes calculated once for all.

In order then to obtain the resistance of the liquid referred to mercury as a unit, the difference of the resistance of the liquid had only to be divided by the difference of the mercury-resistance of every two siphon tubes. With three siphon tubes, therefore, the three possible quotients must give the same numbers. But even with two siphon tubes a simple control

was possible; for, from the difference of the liquid resistances, by the aid of the mercurial resistances the entire resistances of the liquids in the siphon tubes could be calculated without the accessory magnitudes. Deducting these resistances from the observed values, the difference represented the magnitude which was due to the resistance of the conducting vessels and the inequality and polarization of the electrodes. In one and the same experiment this magnitude must be the same for all the siphon tubes used.

By this method I have investigated aqueous solutions of sulphuric acid, sulphates of zinc, copper, and magnesium, and hydrochloric acid. I am indebted to Dr. Rudorff for the chemically pure solutions of known strength. The observations were made at the temperature of the room.

Sulphuric Acid.		
	Temperature.	Resistance as compared with mercury.
H_2SO_4	$15°$ C.	96950
$H_2SO_4 + 14H_2O$	19	14157 ⎫ Minimum.
$H_2SO_4 + 13H_2O$	22	13310 ⎭
$H_2SO_4 + 499H_2O$	22	184773

Sulphate of Zinc.		
$ZnSO_4 + 38H_2O$	23 C.	194400
$ZnSO_4 + 24H_2O$	23	191000 ⎫ Minimum.
$ZnSO_4 + 107H_2O$	23	354000 ⎭

Sulphate of Copper.		
$CuSO_4 + 45H_2O$	22 C.	202410
$CuSO_4 + 105H_2O$	22	339341

Sulphate of Magnesium.		
$MgSO_4 + 34H_2O$	22 C.	199180
$MgSO_4 + 107H_2O$	22	324600

Hydrochloric Acid.		
$HCl + 15H_2O$	23 C.	13626
$HCl + 500H_2O$	23	86679

Although I have examined the acids and salts in question

mixed with many other proportions of water (in which I have had the kind aid of M. Patry of Geneva), I only give the present numbers, as I intend to repeat and extend the observations at constant temperatures.

In reference to the resistance of the mixture of two liquids, assuming that the liquids do not act chemically upon one another, we may either expect the arithmetical mean, or, if the current traverses both liquids separately and therefore divides between them, we may have

$$W = \frac{2R}{1 + \dfrac{1}{m}},$$

where W is the resistance of the mixture, R the resistance of one liquid, m a number which expresses how much greater is the resistance of the second liquid over that of the first, assuming that equal volumes have been taken of both liquids. If, as for example in the case of sulphuric acid and water, m is very large, the resistance of the mixture can at most be double the resistance of the best conductor.

The experiments agree with neither supposition. I mixed, of the liquids to be investigated, first those which were dissolved in equal equivalents of water, and obtained the following results :—

	Resistance of the individual liquids.	Arithmetical mean.	$W = \dfrac{2R}{1 + \dfrac{1}{m}}$	Observed.
$ZnSO^4 + 50 H^2O$ $CuSO^4 + 50 H^2O$	232600 213832	223216	222840	193920
$ZnSO^4 + 50 H^2O$ $H^2SO^4 + 50 H^2O$	232600 25775	129187	46300	64800
$CuSO^4 + 50 H^2O$ $H^2SO^4 + 50 H^2O$	213832 25775	119803	45900	63460
$ZnSO^4 + 23 H^2O$ $CuSO^4 + 55 H^2O$	194400 225254	209827	208700	192430
$ZnSO^4 + 23 H^2O$ $CuSO^4 + 105 H^2O$	194400 339341	266870	247200	199620

The numbers, which might have been somewhat (but not much) different if they had been observed at constant temperatures, show that the resistance of mixtures of two liquids is nearer that of the best conductor.

With reference to the connexion between the conduction of

electricity and heat, experiments which I shall detail in another place show that it does not exist. I investigated mercury, water, sulphuric acid (specific gravity 1·25), concentrated solutions of chloride of sodium, sulphates of zinc and of copper. I find the following series, in which the better conductor precedes :—

Conduction for heat.	Conduction for electricity.
Mercury.	Mercury.
Water.	Sulphuric acid.
Sulphate of copper.	Solution of chloride of sodium.
Sulphuric acid.	Sulphate of zinc.
Sulphate of zinc.	Sulphate of copper.
Solution of chloride of sodium.	Water.

XXXVII. *Fundamental Principles of Molecular Physics.*
By Professor J. Bayma, *S. J., of Stonyhurst College.*

[Continued from p. 188.]

II.

HAVING prepared his ground for the defence and for the attack, Professor Norton comes to my objections against his third and fourth principles, which the reader will find in the Philosophical Magazine (February 1869, p. 99).

Three forms of matter.—I had asked : On what evidence are we to grant that matter exists in three forms essentially different from each other ? The learned Professor gives two answers. The first is that such forms of matter are admitted even by me, though in different words. And to prove this, he remarks that I, in my 'Molecular Mechanics,' admit these three things :— (1) the nucleus of a molecule, (2) the envelope of the nucleus, (3) the universal æther : and from this he infers that the difference of doctrine between us, from the present point of view, is in name only. I am sorry to say that the difference lies not in the name only, but in the thing also.

According to my doctrine, the matter of which the nuclei (I say *nuclei*, not *nucleus*; for there are many in each molecule) are made is exactly of the same form as the matter of which the envelope is composed and the matter which is to be found in luminiferous æther. For in the doctrine of simple elements, which I have advocated, the matter in each element is a single unextended point : and unextended points cannot differ in form.

If however Professor Norton in his notion of matter implies the *force* also (which I doubt, as he in his first principle speaks of *matter* and *force* as of distinct things), then the form of matter would be its principle of activity, which is the formal constituent of material substance. In this case I am ready to allow that

T 2

there are two forms or *kinds* of matter, attractive and repulsive. But this can hardly be his meaning : as he distinguishes *kind* from *form,* and says the kinds are two, whilst he maintains that the forms are three.

Lastly we might understand the word *form* as meaning the result of a kind of composition. But if we accept this meaning, the forms of matter cease to be three, and they become as many as there are primitive compounds in nature. And thus the assumption of three forms of matter can find no explanation in my doctrine, whatever the meaning we attach to the word *form.*

This shows well enough how much Professor Norton is mistaken when he says that I " proved to my own satisfaction that matter does in fact exist in three essentially different forms." I had said indeed that luminiferous æther is " a special substance," in the same sense as I say that oxygen and carbon are special substances, but I never said nor implied that æther was " a distinct form of matter ;" I rather taught the contrary by stating (Molecular Mechanics, p. 174) that æther, though a special substance, was not " a new specific matter."

Professor Norton's second answer runs as follows :

" But to reply to others who may be disposed to adopt the objection urged   No one will deny the existence of gross or ponderable matter, or of something which has all the mechanical attributes of matter.   That an æther exists in space and within transparent media we may certainly regard as abundantly established by optical phenomena.   As to electric æther, the evidence of its existence is that the great body of electric and magnetic phenomena, it is generally conceded, admit of satisfactory explanation on the hypothesis of an electric fluid or æther intimately associated with matter, and that no successful attempt has yet been made to account for the simplest of these phenomena on any other hypothesis."

I beg leave to say that this answer is not to the point.   The assertion to be proved was " that matter exists in three forms essentially different."   Now, in the whole passage the word *form* is not even to be found : and that which has no place in the premisses, cannot find place in the conclusion.

It is not my intention to deny that there is something in nature which corresponds to what Professor Norton calls " gross matter :" yet I believe that the name of gross matter is calculated to engender misconceptions with regard to a subject about which men are already too much biased by the common prejudices of infancy.   Gross matter !   What is it ?   It is said to be that which possesses " all the mechanical attributes of matter." I will not say the contrary : but I may remark that material substance as such has no other mechanical attributes besides activity, passivity, and inertia : and these are to be found in æther

also. Why then should we make a distinction between gross matter and æther, as if the matter of the one had attributes not shared by the matter of the other?

The author will say that *his* " gross matter " has other mechanical attributes besides the three mentioned. True : but I maintain that these other attributes are not attributes *of matter itself* as such, but *of a mechanical compound* of material parts. For it is only on account of its mechanical composition that the so-called " gross matter " possesses different properties in different substances—it being evident that it is a difference either in the number and nature of the components or in the mode of their composition that entails a difference in the constitution, atomic weight, and other properties of molecules. On the contrary, if gross matter were only *one* form of primitive matter, how could it possibly have different attributes in different substances ? Gross matter, according to Professor Norton, is attractive : and yet possesses all the mechanical attributes of matter, and therefore impenetrability, viz. reactivity against pressure ; an attribute which implies repulsive powers. Now, the learned Professor acknowledges that attractive matter and repulsive matter are two *kinds* of matter. He must therefore acknowledge that gross matter is not a primitive form of matter, but a compound of primitive elements, some attractive, and others repulsive. This remark would seem sufficient to expose the fallacy of Professor Norton's argument. He calls " gross matter " that which is not a primitive unit, but a number of material units : and then concludes that such gross matter having special properties must be " an essentially different form of matter." We might as well say : Let a company of men be called " gross man ;" then, as such a gross man has some attributes which are not predicable of each individual, let us conclude that " gross man " is an essentially different form of man.

Something remains to be said about the two æthers, the electric and the luminiferous, which are, according to the author, two other distinct forms of matter. The learned Professor himself remarks that " some physicists are striving to do away with the supposed electric fluid." This fact, which is perfectly true, would tend to prove that the existence of electric æther as a special form of matter is not yet " an established truth." But Professor Norton adds :

" Our author implies in the remarks above quoted that the existence of an electric æther is not only not an established truth, but is to be ranked among those questionable notions that have been received without serious examination. This implication is obviously unjust. Besides, the serious examination that he has given the subject only leads him to confirm the substantial truth of what he would

here seem to discredit; for, as we have already seen, his repulsive envelope is essentially my electric atmosphere."

My preceding remarks about the three forms of matter make any further answer unnecessary. I may, however, add (1) that my repulsive envelope is a most essential part of the molecule, no less indeed than the nuclei: it therefore essentially belongs, according to my views, to what Professor Norton calls "gross matter" or ponderable matter. Then, how could the learned Professor find it to be essentially "his electric æther," which, as he maintains, must be another form of matter essentially different? (2) That my argument was not directed against the existence of some agency on which electric phenomena must depend: such an agency every one is compelled to admit. The point in question was whether the explanation of phenomena required the existence of a *special* form of matter essentially different from that of gross matter and of luminiferous æther. It is the assertion alone of the existence of such a *special* form that I implied to be not only not an established truth, but a very questionable assertion. It may be that the assertion was made by Professor Norton after "serious examination:" yet from what I have stated it would appear that his examination might have been more serious.

I conclude this point by repeating the question which had to be answered: On what evidence are we to grant that matter exists in three forms essentially different from each other? Has Professor Norton given such evidence? The reader will decide.

Two æthers.—My next question was: Why should we admit two æthereal fluids which are both repulsive, and only differ in subtlety? He answers thus:

" Professor Bayma and myself agree in admitting the existence of two *kinds* of matter, attractive and repulsive ; and, as we have seen, three *forms* of matter. Is it inherently any less probable that two of these should be repulsive and one attractive, than, as he assumes, that two should be attractive and one repulsive ?"

I think I have already made it sufficiently clear that I do *not* agree with him about the existence of his three forms of matter. I therefore cannot be bound to admit either that two forms are attractive and one repulsive, or that two forms are repulsive and one attractive. He on the contrary who teaches the existence of three forms of matter and pronounces it to be "an established truth," must be competent to enlighten us as to the grounds on which that truth has been established. In like manner, when the learned Professor tells us that there are two æthereal fluids both repulsive, and we ask: *Why two, since one might suffice*? we should be thankful to obtain a clear and posi-

tive answer from him.   Unluckily he did not give one : at least,
I look in vain for it.   In its stead he gives some short com-
ments upon the differences which I admit to subsist between
what he wrongly calls my "two attractive forms of matter," and
especially upon my opinion on the density of luminiferous æther.
Yet the point at issue was not whether I have succeeded or not
in establishing the great density of luminiferous æther, but how
it can be proved that two æthers both repulsive must be ad-
mitted in nature.

He then says :

" I might also reply to Professor Bayma by asking him why we
should admit, in order to explain electric and optical phenomena,
two substances so distinct as the repulsive envelope of molecules and
the attractive luminiferous æther.   The evidence of their similarity is
much greater than of their dissimilarity."

Why should we admit them ?   The answer is plain.   We
should admit the "repulsive envelope," because we have proved
its existence as an essential part of the molecule (Molecular Me-
chanics, p. 147) : and we should admit an "attractive luminife-
rous æther," because we have also proved its existence by the
consideration of natural facts (Ibid. pp. 176–180).   Had Pro-
fessor Norton done the same with regard to his three forms of
matter, I should never have thought of putting him the ques-
tion *Why should we admit them* ?

From this answer the author will perceive that it is not pre-
cisely or solely in order to explain electric and optical phenomena,
but in order to account for all other phenomena, that the repul-
sive envelope of molecules and the attractive luminiferous æther
are to be admitted.   The repulsive envelope however does not
deserve the name of a special substance ; for it has not an inde-
pendent and complete constitution of its own, and belongs as a
constituent part to the molecule of which it is the envelope.

As to the evidence of its similarity to luminiferous æther,
where is it ?

The learned Professor concludes his answer by these words :

" The only apparent force in the question under consideration is
derived from the fact that a vague conjecture is apt to be raised by
it, that a single æther may be equal to all the duty now assigned to
both."

Be it as he wills.   Yet even a vague conjecture would have
no little weight against an assertion which, as we have shown, is
itself at the best only another vague conjecture.

What Professor Norton adds immediately after has no need of
special reply.   He says that my molecular envelope "is com-
posed of electric matter," and that the universal æther condensed,

as he believes, around each molecule, is "the medium in which pulses are originated that constitute the force of heat repulsion." These views, which in the present controversy have only a secondary importance, I will not now discuss, as much remains to be said upon points of greater moment.

A molecule.—One of my remarks about Professor Norton's theory was that " Had Professor Norton known the impossibility of continuous matter, he would have found out that what he calls an atom of gross matter comprises already not only the central element of a molecule, but its nuclei and its envelope ; and consequently is already *endued with the properties and invested with the arrangements which enable it to exert forces of attraction and repulsion* upon other molecules, without requiring any new and special atmosphere of electric or luminiferous æther." To this he answers :

" That is, in other words, as already shown, Professor **Bayma's** nucleus and envelope are in all outward relations precisely correspondent to my central atom and electric atmosphere. The only essential point of difference between us lies in the fact that I conceive that the interstitial luminiferous æther is condensed around the central atom, and is concerned in the production of some of the phenomena. It is not easy to see how Professor Bayma escapes the conclusion that his interstitial æther, which is attracted by the central nucleus, is condensed around it."

Here the learned Professor intends to show that his theory is in a manner implied in mine, and mine in his, the only difference between us being a difference of words. I heartily wish that such may be the case, as I should deem myself highly honoured by concurring in the views of a man whom I regard as a good authority in science. Unfortunately our present question is not so much one of physics as of philosophy ; our different manner of speaking proceeds in fact from a wide difference of principles. Professor Norton endeavours to attenuate such differences as far as he can : yet they cannot disappear : they remain a sufficient obstacle to our mutual agreement. He speaks of my molecular " nucleus " in the singular, whilst I always speak of molecular " nuclei " in the plural ; the " nucleus " is for him " gross matter " or a special form of matter essentially different from that of his electric æther, whilst my " nuclei " are not a special form of matter essentially different from that of the molecular envelope : his " nucleus " is one piece of continuous matter, my " nuclei " are systems of discrete material points : his " nucleus " is all attractive, my " nuclei " are some attractive and others repulsive, as it appears from the molecular formulas which I give *passim* in my ' Molecular Mechanics.' Now these differences are too substantial and too radical to be passed over in silence. On the

contrary, should we differ only in the question whether or not luminiferous æther is *condensed* around a central atom (or rather around the nuclei) the difference would be comparatively unimportant, as it would bear simply on a point of detail.

Accordingly, when Professor Norton states that this last point is "the only essential point of difference between us," I must deny the truth of a statement which tends to shift the ground of our discussion and to substitute a question of facts for a question of principles. I have shown that the point touched upon by the learned Professor is not the *only* point of difference: and moreover I do not consider it an *essential* point in the present controversy whether I can or cannot "escape the conclusion that *my interstitial æther,* which is attracted by the central nucleus, is condensed around it." Of course, I am confident that I can escape the conclusion; for I have never admitted the existence of any interstitial æther between the nuclei and the envelope of a molecule. But to proceed.

I had expressed my opinion that the examples by which Professor Norton illustrates his theory "do not imply the existence of extended atoms or of two distinct æthereal substances." To this he answers thus:

"When he has shown this to be true of even the ordinary calorific and electric phenomena, we will admit that his objection to a second æthereal atmosphere interpenetrating the first may have some force."

But this is not a sufficient reply. The *onus probandi* evidently rests with him who makes positive assertions, not with him who asks for a proof of them. Now, it is Professor Norton that started his theory by positively affirming that the existence of his three forms of matter was an established truth. If then we happen to find nothing like a sufficient ground for his affirmation, it is not for us to prove that we have found no grounds; it is for him to point them out.

He adds:

"He has given no hint of the general manner in which he supposes electric phenomena to be evolved. Heat he conceives to originate in the vibrations of the molecules of bodies; but it can be proved, almost to a demonstration, that heat cannot originate in this manner."

Electricity is a branch of science scarcely a century old, and electric theories are as yet, as everyone knows, obscure and uncertain. Such being the case, I did not think it convenient to devote to conjectures more or less probable space in a book of so positive a character as I intended my 'Elements of Molecular Mechanics' to remain. Hence it is perfectly true that I gave no hint of the evolution of electric phenomena. But the state-

ment that I conceive heat " to originate " in the vibrations of the
molecules of bodies is not equally correct.   I said indeed that
" the first cause of calorific motion is to be found in the very
constitution of molecules . . . . which are always subject to vibra-
tory motion " (Molecular Mechanics, p. 264) : but the *first*
cause is only one out of many, as nothing is called *first* without
reference to a second.   On the other hand, when speaking of
vibrativity, to which I trace the phenomena of heat, I state that
vibrativity, as a general property of bodies, implies the capability
of making vibrations determined " by an extrinsic agent " (Ibid.
p. 171).   In fact, I conceive heat *to consist*, though not *to origi-
nate*, in certain vibrations of the molecules of bodies, and these
vibrations to depend *first* on the molecular constitution of bodies,
secondly on any extrinsic causality that is brought to bear on
their velocity and intensity.   If Professor Norton proves " almost
to a demonstration " that *this* view is untenable, I shall be thank-
ful to learn in what his proof consists.   Should he however, as
I am afraid he will, argue from the *supposed* existence and work-
ing of universal æther between the molecules of bodies, I would
tell him beforehand that such a proof, besides other considerable
objections, might admit of a reply drawn from the well-known
law of calorific capacities.

Luminiferous æther.—One of my objections against Professor
Norton's theory was that luminiferous æther, according to him,
was a repulsive and resisting medium, which I had shown to be
irreconcilable with astronomical facts.   He answers :

" The principal astronomical fact here referred to is that the
planets do not encounter any sensible resistance in their motion
through space.   The evidence of an æthereal resistance afforded by
Encke's comet, Professor Bayma strives to explain away without
success."

I give to the reader the opportunity of judging for himself of
this point of our controversy, by transcribing the passage referred
to by the learned critic.   " How do we know that Encke's comet
cannot possibly have suffered a change in its orbit, unless it
moves through a resisting medium ? Have we any evidence, or
even any ground whatever for a probable conjecture, that there
is absolutely nothing in interplanetary spaces, except the medium
and those bodies which we have hitherto observed ? To dis-
courage such a supposition it would suffice to mention Le Ver-
rier's discovery.   I do not say that we shall hereafter discover
any new celestial body by whose action to account for the modi-
fication of the orbit of Encke's comet : there are perhaps thou-
sands of bodies in the solar system of which we have no notion,
and never shall have, on account of their being unobservable.   I

say only that with the scanty knowledge we have of the bodies that move in heavenly spaces, and after the discovery of so many new planets, the existence of which had never been suspected before, it would be too rash on the part of a man of science to pronounce that no other cause exists in the heavens to which we can trace the change of Encke's comet course, except a resisting medium. The more so, since Encke's comet shows a regular *acceleration* of its motion, instead of a retardation. Now acceleration does not proclaim, but refute, the theory of a resisting medium ; and therefore Encke's comet and all the other celestial bodies with one loud voice proclaim, and witness in fact, the absolute non-existence of a resisting medium " (Molecular Mechanics, pp. 178, 179).

Professor Norton goes on arguing :

" The fact that no sensible resistance is experienced by the planets does not necessarily imply, as he supposes, that the æther is not repulsive. For, in the first place, if the molecules of the planetary mass have the constitution I have attributed to them, the impinging æther must take effect upon either the æthereal or the electric atmospheres of the molecules, and so may be mostly expended in the generation of heat and electric currents. I have in fact undertaken to show, in my paper on Molecular Physics, that the earth may derive its magnetic condition and a certain portion of its heat from the impact of the æther of space."

In this argument the learned Professor assumes (1) that the molecules of the planetary masses have the constitution which he has attributed to them, and (2) that the impinging æther spends the greatest part of its impetus in producing heat and electric currents. The first of these assumptions has been examined sufficiently, I believe, in the preceding pages. With regard to the second (which apparently would not stand without the first) I may add that, in my opinion, no possible production of heat and electric currents affords a sufficient ground for assuming a reduction of resistance and retardation. Impact is a mechanical fact, the first, direct, immediate result of which is not a production of heat or of electric currents, but simply a communication of motion in a certain direction. Accordingly the impact of æther on a planet primarily and directly tends to check its velocity of translation through space : and only inasmuch as the effect of this effort cannot instantaneously be transmitted from the surface of the planet, where it is exerted, to the remaining molecules of the planetary mass, it gives rise to intermolecular motion. In other words, the generation of heat or of electric currents depends upon the actual diminution of the velocity of translation as a previous condition, intermolecular motion being the result of a preceding change in the mutual relation of mole-

cules, of a change by which the conditions of the molecular equilibrium have been violated. Hence, before we speak of a possible generation of heat and electric currents in the movement of a planet through a resisting medium, we must admit the full efficiency of the impact in retarding the motion of the molecules directly exposed to the resistance of the medium. The generation of heat and electric currents may, or even must, follow, according to the assumption : but that which follows cannot entail the least decrease of intensity in the retardation which precedes, and of which it is the result.

Professor Norton here employs a second argument. He says:

" Again, if the action of gravity be not instantaneous, it will take effect in a direction slightly inclined to the radius vector, and, in the existing state of the planetary system, the tangential component resulting from this inclination may be in equilibrium with the feeble overplus of resistance from the æther."

To this I answer (1) that, if luminiferous æther is supposed to be repulsive, the study of the phenomena of light leaves no room whatever for any hypothesis implying that such æther opposes a *feeble* resistance.   This I have proved in my ' Molecular Mechanics' (pp. 176–180).   As to the *overplus*, I have shown just now that it would be the *whole* of the resistance.   (2) That the hypothesis concerning the *successive* propagation of the action of gravity is utterly false, as I have fully shown in my work (pp. 63–65), and has no other foundation than the confusion of two things perfectly different, viz. action and motion, cause and effect.   (3) That, independently of the two preceding remarks, and granting (only for the sake of the argument) that the said two assumptions might be admitted, it would still be false that the action of gravity " will take a direction slightly inclined to the radius vector " and have " a tangential component."   The case of gravitation is not parallel to that of the aberration of light, as Professor Norton's argument seems to imply.   Gravitation invests *continually* the whole mass of the planet, and its resultant passes through the centre of it : hence no tangential component of gravitation is conceivable consistently with the received principles of general mechanics.

The learned Professor, after the two arguments hitherto examined, gives a third which consists of a retorsion of my difficulty against my own explanation :

" Besides, the supposed difficulty is not removed by substituting an attractive for a repulsive æther.   It is true that when a molecule of the earth's mass encounters an atom of the æther on the line of its advance, it will, upon Professor Bayma's idea, pass through it and leave it behind ; but he has failed to note the fact that, during the

approach of the two, their relative velocity will be equal to the sum of the velocity of the earth and that due to their mutual attraction, and, during their separation, will be equal to the difference of the same velocities, and hence that the atom of æther will continue to attract the molecule during a longer interval of time while the two are se- parating than while they are approaching. The molecule will there- fore on the whole be retarded by the action of the atom. If the attractive æther be 'immensely denser than the atmospheric air,' the resistance should certainly not be less than that of a subtile re- pulsive æther."

This argument has one great defect : it is based on a false alle- gation. It is not "my idea," nor has it ever been, that a mole- cule of the earth's mass passes through the atom of æther and leaves it behind. The earth in its path beats back and carries before it the whole mass of æther upon which it impinges : and the reason why it finds no resistance is not because it leaves the æther behind, but because the æther itself is destitute of all re- pulsive powers. This is a sufficient answer to the learned Pro- fessor's objection. He will see that " by still holding to my line of argument " I cannot be compelled, as he believes, " to abolish the æther of space altogether."

Electric æther.—I had proposed two other questions on the fundamental principles of Professor Norton's theory : the one regarded the material continuity of gross matter, to which he made a long reply, which I intend to examine in a future paper. The other regarded the nature of his electric æther. I had pointed out that this æther, being repulsive, could not attract luminiferous æther ; and yet the author seemed to hold this contradiction.

He replies thus :

" Professor Bayma has here entirely misunderstood me, and re- presented what I threw out as a possible and perhaps probable con- ception to be a fundamental principle of my theory."

To this I have only to answer that I did *not* represent his con- ception as a fundamental principle of his theory. I argued on the contrary that this conception could not agree with the fun- damental principles of his theory. A reference to my 'Molecular Mechanics ' (p. 188) will convince the reader of the truth of my statement. As for misunderstanding him, I regret the fact if true : of course Professor Norton is better qualified than I to explain his own meaning. He says :

" The real fundamental principle was that the atoms of electric æther repelled each other ; and it was merely conjectured that this repulsion might be due to atmospheres of luminiferous æther con- densed around the electric atoms, instead of being a repulsive action."

Very well : but if the conjecture is " a possible and perhaps probable conception," the action of electric æther (prescinding from the surrounding luminiferous æther) is not yet proved to be repulsive. Then the electric æther itself, as distinct from luminiferous æther, might be attractive. What then becomes of the " established truth " and of " the real fundamental principle " that the atoms of electric æther repel each other ? Professor Norton will say that the word " electric æther " includes not only the atom, but also the atmosphere of luminiferous æther condensed around it. But how then is electric æther *one* special form of matter, since it implies *two* ?

All this I did not and do not yet understand : and such being the case, I must confess that it was simply impossible for me to guess at any other meaning that Professor Norton's words might consistently bear, except that which I ventured to express in my objection.

The learned Professor adds :

" It is a little singular, in view of this distinct státement of the manner in which the repulsion might result from a possible attraction, that our author should ask the question, ' Now, if the atoms of electric æther are repulsive, how can they attract ? ' and thereupon intimate the existence of a discrepancy fatal to the theory.''

Professor Norton will at any rate acknowledge that my question will cease to be singular, if I modify it in the following manner :—" If electric æther is *of itself* possibly attractive, how can we assume as an established truth and a fundamental principle that the two æthers differ *only* in subtlety and are both repulsive ? "

He concludes :

" It is, in fact, altogether immaterial whether the mutual repulsion of electric atoms is indirect as conjectured or direct."

I reply that the difference is very material in a theory which teaches the existence of two repulsive forms of matter. For if the electric atoms repel only indirectly, viz. by the help of an atmosphere of luminiferous æther, such repulsion is due to luminiferous æther *alone*. Accordingly the electric æther would not be a second *repulsive* form of matter. This inference cannot be regarded as immaterial by one who maintains the existence of two repulsive æthers differing only in subtlety as an established truth.

To sum up. I have shown, I believe, that Professor Norton's attempt towards solving my objections has proved unsuccessful. Accordingly, though he says in his concluding statement : " It has now been made sufficiently apparent that the objections urged against my theory of Molecular Physics have no real

force, and that its fundamental principles have not been disturbed," I am sorry to think that I am still justified in maintaining the contrary. I leave it to a man of his thoughtful mind to weigh the reasons which compel me, however reluctantly, to differ from him.

<div style="text-align:center">[**To be continued.**]</div>

XXXVIII. *Further Remarks on the Explanation of* Stewart *and* Tait's *Experiments on the Heating of a Disk rotating* in vacuo. *By* OSCAR EMIL MEYER *.

WITH reference to the paper "On the Explanation of Stewart and Tait's Experiments" contained in one of the previous Numbers of the Philosophical Magazine†, the gentlemen in question have published a reply ‡ which I cannot leave unanswered, although I should prefer doing so, for I fear it may lead to a protracted controversy about trifles.

They consider "the assumption that a change of position of the instantaneous axis of the disk necessarily implies loss of *vis viva*" to be "contrary to the usual principles of ordinary dynamics." I consider it to be an application, that was perhaps not indicated clearly enough, of Carnot's theorem, that every sudden change in motion is accompanied by loss of *vis viva*.

Messrs. Stewart and Tait think the calculations based on this assumption "very peculiar," and a little further on they confess "that they cannot pretend to understand them." For this reason they ask for an explanation of the manner in which *vis viva* is transformed into heat. They ask especially whether I "mean that impact of the axle on the bearings may produce vibrations of the disk which in time will by viscosity be frittered down into heat."

I have not expressed my opinion on this point, because I think it to be quite immaterial. The principle of *vires vivæ* has the advantage of holding good quite independently of any consideration as to the mode of the transition of *vires vivæ* out of one state into another. It is sufficient to know the amount of *vis viva* at the commencement and at the end.

If mechanical force be transformed into heat either by means of sonorous or non-sonorous vibrations, by compression, by electricity, by chemical combination, or by any means whatever, in all these cases the law holds good, namely, that the heat gene-

* Communicated by the Author.

† Pogg. *Ann.* Oct. 1868, vol. cxxxv. p. 285. Phil. Mag. Jan. 1869, vol. xxxvii. p. 26.

‡ Pogg. *Ann.* Jan. 1869, vol. cxxxvi. p. 165. Phil. Mag. Feb. 1869, vol. xxxvii. p. 97.

rated is equivalent to the loss of *vis viva*. The question, therefore, is without any bearing upon the present dispute.

Nevertheless I shall not keep back my opinion, as Messrs. Stewart and Tait wish for it. In answer to the question put to me I answer simply, yes. But I do not admit the following statement to be correct :—" he merely repeats one of many objections long ago perceived by ourselves, and also pointed out to us by others, an objection which we have already at least partially met by experiment and calculation." No, not even partially; because there is a difference between my objection and that of Messrs. Stewart and Tait*. They refer to the 250 vibrations per second, which correspond to the fundamental note of their disk. The agitations and impacts I refer to correspond to 2×2500 vibrations in 30 seconds, or to 83 vibrations to and fro per second. The corresponding note would be of a lower pitch than the fundamental one ; and as it would consist simultaneously of longitudinal and transversal vibrations, it cannot exist, and therefore must appear as heat.

The reply of Messrs. Stewart and Tait contains only a single positive argument against my explanation of the experiment. Placing too implicit confidence in the accuracy of a figure adjoined to one of the papers†, I made a mistake as to the length of the axis. This may invalidate my supposition that the vibrations were produced by impacts of the axle upon the bearings. But this supposition is not essential. What is essential to my explanation is only the conception of impulses occurring periodically and twice during each revolution of the disk. The irregularity in the rotation of the disk can be explained just as well by supposing that the axle had a slight bend, or that the pinion attached to it was slightly excentric. Such irregularities produce by impulses sudden changes in the speed of the rotation. Alternately the velocity increases and decreases ; the excentric wheels and pinions striking one another and then again being thrown asunder. My assumption, therefore, of impulses occurring periodically backwards and forwards, remains intact, as also the calculation ; and I maintain that my explanation, in spite of the error mentioned, is still correct.

On the other hand I admit, without reserve, that my explanation is not positively proved. Still it can be very easily verified, for instance by repeating the observations with a different velocity of rotation. And as both gentlemen are about to make new experiments with regard to the objections raised by Professor Helmholtz, I would request them kindly to take into considera-

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* Art. 20 of the third paper.   Proc. Roy. Soc. vol. xv. p. 295.   Phil. Mag. S. 4. vol. xxxiii. p. 230.

† Proc. Roy. Soc. vol. xiv. p. 339.   Phil. Mag. S. 4. vol. xxx. p. 314.

tion my objection as well. Should the experimental test decide against me, I will willingly retract my calculations, the more so because they are only approximately correct. This may be brought about either by the new experiments of Messrs. Stewart and Tait, or by Sir William Thomson's promised determination of the radiation of heat in absolute measure. But so long as this experimental test is wanting it would be idle to continue a dispute about an unproved hypothesis.

Breslau, February 13, 1869.

XXXIX. *On the Falsetto or Head-Sounds of the Human Voice. By* WILLIAM MARCET, *M.D., F.R.S., Assistant Physician to the Hospital for Consumption and Diseases of the Chest, Brompton**.

THE consideration of the cause of the head-notes of the human voice is of much interest, not only in a physiological point of view, with the object of accounting for the production of those wonderfully soft and penetrating sounds in which Swiss and Tyrolese singers are so remarkably proficient, but also for the purpose of explaining certain morbid changes of the human voice not unfrequently met with. Let me premise that, by means of the laryngoscope (a small mirror so constructed as to allow the observer to view the inside of the larynx and the action of its various parts in the production of sound), the phenomenon of phonation can be investigated with great minuteness. There are, moreover, certain changes of form the larynx undergoes in the act of phonation, and bearing on the subject of the present communication, which can be determined by external observation, much in the same way as we can ascertain by looking and feeling, that the windpipe is raised in the act of swallowing.

The vocal apparatus consists of two folds of the mucous membrane of the larynx, supplied at their free edge with bands of elastic tissue. These folds are opposite each other in a horizontal plane, and in the antero-posterior axis of the larynx. Their lips or free edges may be brought so close as to touch one another, while at the same time they may be subjected to a certain degree of tension; if under these circumstances air be blown out from the lungs, these laryngeal folds or *vocal cords* will be made to vibrate, emitting a sound.

It is considered by J. Müller and others that the human chest-sounds are owing to the whole breadth of the laryngeal membranes entering into vibration, while the head-sounds are due to the vibrations being confined to the margins or mere edges of the membranes. Helmholtz thinks the head-sounds are caused

* Communicated by the Author.

by the removal of the mucus which constantly moistens the rim of the cords ; their edges thus become sharper and their weight less, while, their elasticity remaining the same, they are shaken into more rapid tremors* The observations I have had the opportunity of making appear to show that the head- or falsetto sounds are really caused by the vibrations being confined to the rim of the cords ; and I have been able to determine with the laryngoscope the conditions of the larynx which give rise to these sounds.

When the larynx is examined with the laryngoscope during the act of phonation, the vocal cords are seen to approach each other *throughout their whole length* and *parallel with each other*, till they appear in contact, or very nearly so. Any circumstance which interferes with this mutual approximation of the cords, such as weakness of certain of their muscles, as shown by Dr. Morell Mackenzie, or swelling of some parts of the larynx, causes the voice to become very weak or entirely suppressed ; and this is a frequent source of aphonia. When the cords do not meet in their entire length, either an anterior or posterior portion of one of them remaining apart, a sound is produced,. but instead of a fair chest-note we have a falsetto or head-sound.

If a finger be applied to the throat, a depression will be felt between the thyroid and cricoid cartilages ; now on singing the notes of the scale, beginning with the lowest, this depression will be felt to contract, on account of one of the cartilages approaching the other, until with the highest notes it has entirely disappeared—this being due to the action of two of the laryngeal muscles (the crico-thyroid), which tightens the cords, thereby raising the sound of the notes. If, however, in imitation of the Swiss and Tyrolese singers, a chest-note should be followed by the same note sung in a falsetto tone, the space on which the finger rests will be found to have undergone little or no contraction, notwithstanding the pitch of the head-sound being no less than one octave higher.

My attention was drawn to the present subject by the remarkable condition of the voice of one of my patients at the Consumption-Hospital. He spoke in a sharp clear head-voice, which consisted entirely of "harmonic" sounds. His larynx, of which I obtained a good view with the laryngoscope, exhibited this very peculiar appearance—that the left cord was bent at about the middle, its anterior half coming in contact with the corresponding half of the other, while the posterior half remained aside, so that the vibrating-power of the blast from the chest was exclusively exerted on the anterior halves of the cords. On applying certain solutions to the cords with a camel-hair brush, I succeeded

* Tyndall, ' Lectures on Sound.'

in bringing back immediately a fair chest-sound, the difference being so remarkable that anybody in the next room could not have believed the same person was speaking; an inspection of the larynx now showed both cords to meet well throughout their whole length. For some time, however, my patient did not retain his chest-voice; so that I had several opportunities of confirming the correctness of this observation. I then lost sight of him for some weeks, during which time his voice remained good; but he finally again applied to me, his speech having relapsed into the falsetto sound. I now observed with the laryngoscope that the anterior half of the left cord was at fault, a chink remaining visible between the anterior portions of the cords during the act of phonation. On the application to the larynx of a solution of iodine in olive-oil, the chest-voice was again brought back, and both cords were seen to come into mutual contact throughout their whole length. The cause of this relapse was due to a swelling of the left side of the larynx (left false cord), extending to the true cord and interfering with its movements. I find in Sir Duncan Gibbs's book on diseases of the throat the account of two cases of what he calls double voice, apparently due to a similar circumstance. In one of them the motion of the left vocal cord was sluggish, slowly coming into action, approximation beginning more at its posterior third. In the other the left vocal cord was narrowed and contracted near its attachment to the arytænoid cartilage, and did not meet its fellow at that situation during phonation.

A case of cracked voice, due to a state of swelling of the cords, has also come under my notice; but the falsetto sound was not so well marked as it had been in the other instance.

I now thought of inspecting the throat of a professional Tyrolese singer while singing falsetto or head-notes; and one of the well-known Tyrolese artists now giving concerts at St. James's Hall, and possessed of the power of singing head-notes in a high degree of perfection, kindly acceded to my request to submit himself to a laryngoscopic examination. I had a fair view of his larynx; but the irritability of his throat prevented me from making so close an examination as I should have liked. His vocal cords were beautifully developed, and the action of his laryngeal muscles was full and rapid; he had some little difficulty, as might have been expected, in singing with the laryngeal mirror in the mouth; still he succeeded with his head-notes. I could then see the cords considerably shortened and applied tightly against each other throughout the whole of their length; this was attended with a rapid play of the laryngeal muscles.

How are we now to apply the facts reported in the present

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communication to the explanation of the mode of formation of head-sounds ?

We must first admit that clear falsetto or head-notes are harmonic sounds ; then we must consider how harmonic notes can be produced by the laryngeal instrument.

If the vocal cords be regarded in the same light as the vibrating "tongue" of a reed-instrument, their nodes* must be parallel to their edges ; when the whole breadth of the cords vibrates, the harmonics are not heard, and the sound emitted is said to be from the chest ; but if the cords meet only on a portion of their length, then the vibrating-power of the blast will be entirely spent in distributing the vibrations in a longitudinal direction to those portions of the cords which are kept apart. Hence the edges only of the cords will vibrate ; the vibrating-body may then be regarded as a narrow strip of elastic membrane included between the edge of the cords and one of the nodes nearest and parallel to that edge. An harmonic note, or a head- or falsetto sound, will be the result of these vibrations. Now this head-sound may be clear, sharp, and well defined, or it may be cracked and even painful to hear. This appears to me due to the circumstance that the vocal apparatus, being reduced to a narrow strip limited on one side by its edge and on the other by a node, becomes a true vibrating cord with nodes in a longitudinal direction. If the cords meet in such a way as to limit the direct action of the blast to the distance between two of their corresponding ends and two of their corresponding nodes, an harmonic sound will be emitted. In fact there will then be a combination of the two harmonics, viz. the one belonging to the vocal cord considered as a membrane, the other to the cord considered as a true cord ; the result will be the emission of a fine, clear, sharp head-note.

But if the cords do not meet in such a way as to limit the direct action of the blast, as stated above, then the sound produced will be the same as that obtained when the exact spot to bring out the harmonic of a violoncello-cord, for instance, is not detected at once, the shrieking disagreeable sound of the instrument showing the performer's skill to be open to much improvement.

The act of singing at will head-notes equally results in the emission of harmonic sounds ; but its mechanism is not quite the same as in the former case. I could not satisfy myself, as I should have wished, as to the muscular action which brought about the shortening and tight approximation of the vocal cords

* It is hardly necessary for me to observe that the "nodes" of a vibrating-cord are the places on the cord which, when lightly pressed with a finger, yield harmonic sounds.

of the Tyrolese singer; but I believe it can be due to no other cause than the close apposition of the arytænoid cartilages through the action of a special muscle, the arytænoideus proprius; and this would account for the formation of harmonics.   The vocal cord may be regarded as fixed to these two cartilages poste- riorly, which in some measure act as parts of the cords; so that these two bodies, by pressing against each other, will reduce the length of the cords, very much in the same way as when a finger is applied lightly to a vibrating violoncello-cord.   The skill of the singer who wishes to pass very quickly from a chest to a head-note, and to strike the right clear sharp falsetto sound, will be an acquired dexterity of regulating the motion of the arytænoid cartilages so as to cause the air from the chest to act on the length of the cords required for one of their harmonics.   If he should in any way miss the exact spot, a cracked sound will be emitted. This will account for the present mode of singing requiring a great deal of practice to be done in perfection.   The fact that the cricoid and thyroid cartilages remain nearly motionless, on passing from a chest- to its corresponding falsetto note, would appear to show that falsetto-singing is not due to increased ten- sion of the cords*.

I have observed that the mere swelling of the vocal cord may produce a harsh head-note; this appears to me to be due to the lessened vibrating-property of the cords, the vibrations being prevented from reaching the whole of their breadth.   I believe this is not an unfrequent cause of cracked voice from inflamma- tion of the larynx, and that it is not an affection difficult to cure.

XL. *On the Absorption of Light by the Air.*
By H. WILD†.

ATMOSPHERIC air, like other ponderable bodies, is not to be considered a perfectly transparent substance, but, espe- cially in strata of some thickness, exerts an appreciable absorp- tion on transmitted light.   Daily experience teaches that this absorption is very different at different times.   Distant objects sometimes appear indistinct and hazy in their outlines, as if they were covered with a veil; sometimes their details are seen so clearly defined that we are unavoidably led to regard them as brought nearer to us.   The first phenomenon is generally ob-

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* It is not impossible, however, that a combined action of the arytænoi- deus proprius and posterior crico-arytænoid muscles should add to the ten- sion of the vocal cords in falsetto singing.

† Translated from Poggendorff's *Annalen*, No. 8, 1868, having been read before the Naturforschende Gesellschaft of Bern.

served in continuously dry weather, while increased transpa-rency is regarded as a sign of approaching wet weather.

With these facts and observations are connected the two prin-cipal views which at different times have been propounded as to the greater or less transparency of the air. A. De la Rive ascribes the less transparency of the air in dry weather to the presence of opaque dust and vegetable germs. If the air becomes moist by south-west winds setting in, these corpuscles become more trans-parent, and at the same time heavier, owing to the absorption of aqueous vapour, so that they fall to the ground more rapidly, which is more completely the case when the rain begins; the air thereby becomes purified and at the same time more transparent. Marshal Vaillant, on the contrary, assigns as the chief reason for the different degrees of transparency in the air in north-east and south-westerly winds, the circumstance that during the pre-valence of the latter, owing to the uniformity in the temperature of the ground and the air, local vertical currents are far less than with north-easterly winds. Disturbed air, however, is far more opaque, because at the boundary of warmer and colder layers of air multifold reflections and irregular refractions of light take place.

We will add no new hypotheses to these, but at once inquire how far this question as to the various degrees of transparency of the air and the absolute magnitude of the absorption of light may be experimentally answered.

Saussure was the first who attempted to measure the trans-parency of the air. He devised for this purpose a simple instru-ment, which he called the *diaphanometer*[*]. It consists of a black circle in the centre of a white circle of three times its diameter. In determining the transparency of the air two such disks are necessary, one of which has a considerably greater (twelve times, for instance) diameter than the other. If either of these disks be gradually moved away from the eye, a point is ultimately reached at which the black central spot disappears. This will be the case when the visual angle of the black circle has become less than the limiting angle of distinct vision, which amounts to about $50''$. If we were merely concerned with this limiting angle, the black spot of the larger disk would obviously disappear at twelve times the distance of the smaller disk from the observer. If, then, as is actually the case, the spot of the larger disk disappears at even a smaller distance, the brightness of the white background has an influence on this disappearance, and the deviation in the ratio of the two distances at which the black circles of the smaller and the larger disks disappear, from the ratio 1 : 12, may serve as a mea-sure for the transparency of the air. From known photome-trical principles, the two white disks uniformly illuminated by

[*] *Mémoires de l'Académie de Turin*, vol. iv. (1789) p. 425.

the sun or diffused light should appear equally bright to the eye of the observer at all distances; if, therefore, the more distant one is less bright, this arises from an enfeeblement of the light in passing through a layer of air twelve times as thick. The more strongly the air absorbs, the more will the ratio of the distances differ from the true theoretical one of 1 : 12.

Beer was the first to give a theory of the diaphanometer based on the principles of photometry[*]. He assumes that the phenomenon would not be materially altered if the disks consisted of white circles on a black ground, and that in this case the two different-sized white spots would send an equal amount of light to the eye at the moment of their disappearance. In fact, according to this view also, the disk twelve times as large would, at twelve times the distance, send to the observer's eye the same amount of light as the smaller one, and disappear simultaneously with this, only if the air were quite transparent. If it is not so, the light which is lost by absorption in the thicker layer of air must be compensated by a greater apparent diameter of the disk. On this assumption the coefficient of transparency[†] a of the air (that is, the fraction of the incident light which traverses a layer of air of the unit thickness) may, according to Beer, be calculated from observations with the diaphanometer by the formula

$$a = \left(\frac{d \cdot E}{D \cdot e} \right)^{\frac{2}{E-e}},$$

in which d and D are the diameters of the two disks, e and E the distances at which their spots just disappear.

By this formula Beer has calculated the coefficient of transparency of the air at two different heights, from observations made on the Tyrolese Alps by H. Schlagintweit with a Saussure's diaphanometer[‡]. He finds that, referred to a unit of length of 1000 Paris feet, at

$$\begin{array}{lll} 2300 \text{ feet above the sea} & . & . & . & a = 0 \cdot 9029 \\ 12000 \quad ,, \quad\quad ,, & & . & . & . & a = 0 \cdot 9985 \end{array}$$

The development of the above formula of Beer necessitates two suppositions which call for a more minute discussion. It is as-

sumed, first, that the two disks are just equally illuminated, and then that the apertures of the pupils of the observer's eyes are equal in looking at both disks. The first condition can in fact only be realized by taking extraordinary precautions ; and the second is, strictly speaking, never fulfilled. It is one of the oldest observations in reference to the accommodation of the eye, that the pupil becomes narrower when accommodated for near objects, and enlarged on looking at a distance. Allowing for this alteration of the pupil, the above formula becomes more exactly

$$a = \left(\frac{d \cdot E}{D \cdot e} \cdot \frac{\delta}{\Delta} \right)^{\frac{2}{E-e}};$$

where δ is the diameter of the aperture of the pupil on looking at the nearer disk, and Δ that for the more distant one. In order to ascertain the approximate value of the factor of correction $\left(\frac{\delta}{\Delta} \right)^{\frac{2}{E-e}}$, I estimated the alteration in the pupillary aperture on passing from the accommodation to an object at a distance of 200 feet to one at 2000 feet, and I found that the diameter changes about 0·6 millim. If, therefore, we put $\delta = 2·4$ millims., $\Delta = 3$ millims. Introducing these numbers into the above equation, in calculating the coefficient a from the observation at a height of 2000 feet we get, instead of the above value $a = 0·9029$,

$$a = 0·7225.$$

The influence of the alteration in the pupil is therefore considerable.

These circumstances, as well as the uncertainty in ascertaining the exact point of disappearance of the black circle, diminish the value of the diaphanometer as a measuring-instrument so materially that it no longer comes up to the present requirements of science.

A. De la Rive has recently again taken up the investigation of the transparency of the air, and followed the only rational path— that of endeavouring to determine photometrically the ratio of the transparency of two objects at different distances from the observer. The instrument in question was exhibited at the Meeting of the Swiss Naturalists in Geneva in August 1865 ; and at the same time the above theory of the causes of the varying degree of transparency of the air was propounded*. De la Rive has not, however, as yet published any results of observation.

Before I knew of De la Rive's work, having become possessed of some long wide tubes for other investigations, I made on the

* Compare Phil. Mag. S. 4. vol. xxxiv. p. 241.

8th of March, 1866, a long projected experiment for measuring the absorption of the air by means of my photometer, which gave, however, a negative result. I shall subsequently revert to it. I at first concluded from it that the transparency of the air was so great that the enfeeblement of the light in a layer a metre in thickness was below the limit of the delicacy of my instrument—that is, did not amount to $\frac{1}{1000}$ of the incident light. I therefore determined to make measurements in the open air at far greater distances. After some preliminary experiments which indicated the precautions to be taken, the definitive observations were made on the 6th to the 10th of July 1866, first in my garden, and afterwards in an open street in the neighbourhood of my house. Two paper screens, consisting of square wooden frames respectively 0·6 metre and 1·2 metre in the side, over which was stretched paper from the same roll, were first put up side by side at a distance of 6 metres from the two apertures in my photometer** by which light enters, and the ratio of their brightness was determined. Without altering the position of the smaller one, I first placed the larger screen at a distance of 21 metres and then of 36 metres from the photometer, again determined the relative brightness by its means, which operation was once more repeated after the screens had been again placed at equal distances. The mean of the first and last measurements (with an equal distance of the screens), compared with the results of the second observation (when the screens were respectively 15 and 30 metres apart), rendered it possible to determine the coefficient of transparency a of the air. For if we designate by e the constant distance of the small screen from the photometer, and the varying one of the larger screen by E, if, further, the brightness of the small screen be called i and that of the larger one J, looking from the photometer, the ratio of brightness of the two is

$$H = \frac{i \cdot a^e}{J \cdot a^E}.$$

The same ratio of brightness of the two screens may be calculated from the angle of neutralization v, read off on the photometer, from the formula

$$H = C \cdot \tan v^2,$$

in which C is a constant depending on the position of the prisms, which is unknown, and has therefore first to be determined by experiment. We have thus now the equation

$$\frac{i \cdot a^e}{J \cdot a^E} = C \tan v^2,$$

* Poggendorff's *Annalen*, vol. cxviii. p. 193.

or

$$a^{e-\mathrm{E}} = \frac{\mathrm{J}}{i} \cdot \mathrm{C} \cdot \tan v^2 \,;$$

and in this case the two unknown magnitudes C and $\frac{\mathrm{J}}{i}$ are defined or eliminated by our placing both disks at the same distance (that is, making $\mathrm{E} = e$), on the assumption that the removal of the larger screen does not alter the ratio $\frac{\mathrm{J}}{i}$. If the angle of neutralization observed in this position is v_1, we have for this case

$$1 = \frac{\mathrm{J}}{i} \, \mathrm{C} \cdot \tan v_1^2.$$

This equation, divided by the above, gives

$$a = \sqrt[\mathrm{E}-e]{\frac{\tan^2 v_1}{\tan^2 v}}.$$

On the above-mentioned days I succeeded in obtaining seven complete observations uninterrupted by any disturbing influences. The unexpectedly large difference in the angles of neutralization v and v_1 (about $2°$ when the screens were at the greater distance 30 metres apart, while I had only expected about $\frac{1}{4}°$) led me to fear that there might have been some disturbing reflection from the house-walls on one of the screens. Hence on the 10th of July the measurements were repeated on the open road (one, that is to say, bounded on both sides by meadows); these, however, only confirmed the previous results. Introducing the values obtained for v and v_1, and the corresponding ones for $\mathrm{E} - e$, into the above formula, we get as the mean of all observations *for the coefficient of transparency of air, referred to* 1 *metre as the unit of thickness,*

$$a = 0.9061,$$

with a probable error of ± 0.0005. This number refers to white light (that is, to the brightest rays in it, the colours that are between Fraunhofer's lines D and E of the solar spectrum), for a mean temperature of the air $= 24°$ C., relative moisture $= 0.55$, and a mean pressure of 722 millims. The layer of air was about 1.2 metre above the ground.

Before making any further observations on this result, I must briefly mention the precautions which are indispensable for accurate observations. It is first of all necessary that during an experiment the relative illumination $\frac{i}{\mathrm{J}}$ of the screens remain exactly equal. For this purpose the larger screen must always be moved quite parallel, which is best effected by fixing a sight

to its frame. The sky must also be almost cloudless; at any rate there must be no mist in the neighbourhood of the sun. Owing to small clouds which passed over the sun, I had frequently to break off observations which had been commenced; for irregular and sometimes very considerable changes ensued in the relative brightness of the two screens. It was also impossible to observe when the wind was at all powerful; for the larger screen more especially was altered in its position and in its illumination. Reflection from adjacent objects, too, which affects the two screens differently, must be carefully avoided.

The above values, when reduced by Beer's formula, and by my modification of it, to the unit of 1000 Paris feet, and, on the other hand, to the unit I have adopted of 1 metre, give us the comparison—

	For 1000 Paris feet.	For 1 metre.
Schlagintweit-Beer	$a = 0.9029$	$a = 0.9997$
Schlagintweit-Wild	$a = 0.7525$	$a = 0.9990$
Wild	$a = 0.2801$	$a = 0.9961$

Our comparatively much smaller coefficient of transparency of the air is partially to be explained by the circumstance that in our observations all those influences which are usually considered to favour absorption were represented. On the days of observation a north-east wind prevailed which at times was powerful; the air was very dry and its temperature high, so that undoubtedly local ascending currents set in, and abundant dust and vegetable germs were met with in the lower layers.

How great is the influence of dust in transparent liquids on their power of absorption has been already shown by my determinations of the *absorption of water*[*]. According as the water was more or less freed from admixed dust by filtration through various kinds of paper, were different values obtained for its coefficient of transparency. *Reduced to 1 metre as unit of path*, we get from these determinations for the coefficient of transparency of water after filtration through

Coarse filtering-paper	. .	0.5368
Medium fine	. . . .	0.6491
Finest	. . . . . .	0.7978

These numbers differ, therefore, comparatively far more than those for air for the same unit of path. It must, on the other hand, be taken into account that in our determinations of the coefficients of transparency of air in the open the particles of dust are also illuminated, and thus not only act as absorbents

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[*] Pogg. *Ann.* vol. xcix. p. 272.

but also as illuminators, while when air or liquids are enclosed in tubes their absorptive action alone comes into play.

The unexpected magnitude of the absorption of the air as deduced from my measurements led me to investigate whether the transparency of the air for different colours could not be experimentally determined in an analogous manner. With this view the relative brightness of the two screens while at a distance of 30 metres was determined—in one case for red light by placing a deep-red glass in front of the ocular of the photometer, in the other case for blue light by the use of a combination of a cobalt with a bluish-green glass. Two sets of observations made in this manner, on the 9th and 14th of September, showed that red light is absorbed less strongly than blue, *or that the coefficient of transparency of the air for red light is greater than for blue.* I do not give the numerical values for the coefficients, as they are only very approximately determined, and may be essentially modified by later, more accurate, and more complete observations.

I was prevented by pressing business from resuming these observations; and only in August 1867 could I proceed to attempt to make them by a new method of observation, which was more in accordance with that which I had first chosen. With the delicacy of my photometer and the unexpected magnitude of the absorption of the air, it seemed that I ought to be able to demonstrate it by means of tubes of about 2 metres in length.

The apparatus which I used consisted of an oiled transparent paper disk of 30 centims. diameter, which was placed near a window only illuminated by diffused daylight, and which, in order to obtain as uniform illumination as possible, was rotated by an axis passing through its centre which was moved by clockwork. At right angles to the face of this disk, on a horizontal diameter of it, and with their centres opposite, two horizontal sheet-metal tubes were placed of 2·4 metres length and 0·1 metre diameter. They were provided throughout their entire length with twelve diaphragms, each with an aperture of 0·06 metre, and were moreover closed at the ends with plate glass. Each had two smaller lateral apertures, which in the case of one tube were constantly open; while one of the apertures in the second was closed, and the other connected with an air-pump by means of a caoutchouc tube with an enclosed spiral. The photometer was placed in front of the ends of the tubes furthest from the paper disk, in such a manner that the light from the two halves of the paper disk could pass through both tubes into the two apertures of the prism apparatus. The chief difficulty and uncertainty in this form of the experiment arises from its being almost impossible to keep the two halves of the disk at a constant ratio of brightness. I could only effect this for the

duration of the same series of observations by shortening them as much as possible, and choosing days when the cloudiness did not much vary. The position of neutralization was observed in only one quadrant, and first of all when one tube was exhausted and the other filled with air, and then after air had been allowed rapidly to enter again. The results to be obtained from these observations are therefore only to be regarded as first approximations. This comparison, moreover, of the ratio of the luminous intensity when the tube was full in one case and in the other exhausted, merely gives us the ratio of the coefficient of transparency of rarefied air, and of air of ordinary density.

The series of observations on the 29th of August gave as the mean angular difference of the two positions on the photometer 21′, which corresponds to a ratio of the coefficients of transparency of the air for pressures of 35 and 720 millims., referred to a length of 1 millim.,

$$\frac{a_{35}}{a_{720}} = 1{\cdot}01023.$$

This result, however, is uncertain, because on this day during the time of observation the clouds, and therewith the illumination of our disk, greatly varied.

On the 31st of August, when the sky was almost cloudless and the illumination almost constant, for four successive series of observations the mean angular difference in the two positions of the photometer was 8′·5, the pressures being 715 and 100 millims. From this we get

$$\frac{a_{100}}{a_{715}} = 1{\cdot}00413.$$

It is here assumed that the increase in the luminosity when the tube was exhausted was exclusively due to diminished absorption, and was not due to a simultaneous diminution of the enfeeblement of the light owing to its passage through the enclosing plates. Fresnel's formulæ of intensity show in fact that transmitted light is not materially altered in its intensity when the glass plates are bounded on one side by a vacuum instead of on both sides by air.

The above numbers may also be used to calculate the coefficients of transparency of air of ordinary density.

The coefficient of transparency of air whose density is $\frac{1}{n}$ that of ordinary air is obviously

$$a_{\frac{1}{n}} = a^{\frac{1}{n}};$$

and since the densities are as the pressures, we shall have

$$n = \frac{P}{p}$$

if P represent the original pressure, and p the pressure of the rarefied air ; thus we have also

$$a_p = a_P^{\frac{p}{P}},$$

where a_p and a_P represent the coefficients of transparency of the air for the respective pressures p and P.

If we put the ratio determined by our previous experiments, of

$\frac{a_p}{a_P} = x$, we have also $a_P = x^{-\frac{P}{P-p}}.$

Introducing x, P, and p, the above values, we get, referred to a unit of path of 1 metre,

$$a_{720} = 0{\cdot}98935 \quad \text{August 29.}$$
$$a_{715} = 0{\cdot}99521 \qquad ,, \quad 31.$$

The latter, which from the reasons stated is the more reliable result, almost agrees within the limits of error with that previously ascertained. Without prematurely attributing too great weight to this agreement, we may draw the conclusion *that by means of my photometer it is possible to determine the absorption of air in tubes of no great length,* those, for instance, which can be set up in an ordinary room.

From the previous observations it follows with certainty that air in the lower layers is far less transparent than is ordinarily assumed. In the case of air only half saturated with aqueous vapour and heated to about 24° C., the absorption near the ground is so considerable, that after traversing 300 metres the luminous intensity has been diminished to about one-third. However paradoxical and large this absorption of air may seem, it is infinitely small compared with that of water. From the above numbers we find that, after traversing the same distance in water as pure as possible, the luminous intensity would only amount to one quintillionth (that is, absolute darkness would set in), and that, to diminish the intensity to only one-third, its path in water need not be more than 5 metres. From my observations I can also deduce with certainty a greater degree of transparency for red than for blue light, though I cannot yet consider the quantitative difference definitely settled. This supports the ordinary explanation of morning and evening red ; yet only further detailed observations can show whether Clausius's theory of morning and evening red, as well as of the blue colour of the sky, which has been accepted by most physicists, is correct or not.

Appendix.

In close connexion with the preceding investigation is another, of which I wish here to communicate a preliminary result. It refers to the difference in the colour of the waters of lakes and rivers in summer and winter, as well as of the warm and saline water of the Gulf-stream as compared with the surrounding water. I consider that the darker or deeper and more vivid colour in summer, and in the Gulf-stream, is not to be ascribed to a greater quantity of salt, but essentially to the higher temperature. It is well known that in most bodies absorption increases with increase of temperature. To decide whether this is also the case with water, I first determined the coloration of a paper surface illuminated by the sun, when looked at through a layer of water 2·4 metres in thickness. To judge the colour the better, the tube was only half filled, so that the uncoloured white paper above was visible. Ordinary well-water cooled to 7° C. gave a bright greenish-white tone, and at 50° C. a decidedly deeper bright green coloration; distilled water, too, at 20° C. exhibited a bright bluish-green colour, which at 58° C. passed into a deeper and greener colour.

The transparency of water at two different temperatures was directly measured. Between the photometer and the ground-glass pane of a front window, at one aperture two glass tubes (each closed by glass plates) of 50 millims. and 200 millims. length were placed, at the other aperture of the photometer two other such tubes of 100 millims. and 150 millims. length. The relative intensities with the empty and the partially filled tubes were ascertained; in one case the tubes of 50 millims. and 150 millims., and in the other those of 100 millims. and 200 millims. were filled with distilled water filtered through coarse filtering-paper. The temperature of the water, of about 6° C., was obtained by cooling the entire room to this temperature; with the higher temperature of 25° C., the room was at a temperature of 16° C. From the experiments made on the 4th and 5th of January, 1867, the following values were obtained for the coefficient of transparency of distilled water filtered through coarse paper:—

$$\text{At } 24°\!\cdot\!4 \text{ C. } a = 0\!\cdot\!91790,$$
$$\text{At } 6°\!\cdot\!2 \text{ C. } a = 0\!\cdot\!94769,$$

referred to 1 decim. as unit of path. My former experiments in Königsberg had given for the same paper and a temperature of about 17° C., and referred to the same unit of path, the value

$$17° \text{ C. } a = 0\!\cdot\!93968,$$

which agrees well with the above. Hence the transparency of water at low temperatures is in fact greater than at higher ones.

XLI. *Notices respecting New Books.*

An Introduction to Scientific Chemistry; designed for the use of Schools and Candidates for University Matriculation Examinations. By F. S. BARFF, *M.A.* London: Groombridge. (Pp. xv and 315.)

THE appearance of this book may be regarded as one of the immediate results of the recent impulse in favour of teaching experimental science, which has been felt in almost every school in this country. Two or three years ago common class instruction in chemistry or physics could only be obtained in a few institutions of the larger and more expensive kind; but it is now understood that these subjects can be beneficially taught to boys, nay, that they are essential elements in a liberal education. Acting upon this belief (in which he has our hearty concurrence), the schoolmaster is naturally exposed to two dangers, which are not inconsiderable, and really deserve his serious attention.

The first danger which the schoolmaster has to encounter is, that he does not, as a rule, appreciate the real value of what he is about to undertake. His own learning has been, for the most part, acquired from books and disseminated by the pen; the methods of accurate observation and of analytical and synthetical reasoning with which experimental science is specially associated, appear to him to have been much overestimated. It will be more economical, and quite as salutary, if, after the little study that may be necessary, he teach the new branches himself. Need we say that such a plan must inevitably end in failure? Yet it is one that has been adopted on several occasions by teachers who would be ashamed to meet a class in mathematics, for example, without being far more than adequate to their duty. It is evident that a science can only be properly taught by some one who is as conversant with it as can fairly be expected from a teacher in any other subject; and care ought to be taken that no one should be employed for this purpose who cannot give proof of at least two years' exclusive attention to the study of the science he professes. We must add that, in addition to a properly qualified instructor, a laboratory is indispensable. To say nothing of the intense delight with which most boys regard laboratory work, it is reasonable to believe that they will miss the very aim of their efforts unless they learn in a practical way that which derives its chief advantage from being practical.

But the twin peril advances from another quarter. If it is true that the average schoolmaster is not sufficiently serious and appreciative, it is no less certain that physicists, and especially chemists, have provided very badly for the event of his becoming so. When he anxiously asks us for the class-book which is to tell as plain and simple a story as he already possesses in the 'Geography' or 'Arithmetic' he uses (or a dozen others that he might as well use), he puts, it must be admitted, a very puzzling question. We are compelled to confess that, even including the manuals which have been expressly

written with this design, there are scarcely any that we can recom- mend as a school-book. Too often devoted to the exposition of the fancies or whims of the hour, or to a wonderful confusion of the creeds of independent apostles, most of our manuals of chemistry are like copies of some manuscript legend in which the original sentences have been repeatedly misplaced and rendered almost un- intelligible by the intervening conjectures of successive readers. Is this the romance of nature ? So much censure certainly does not attach to the current physical manuals ; but they are almost all far above a boy's level, and in a form unavailable for a teacher.

Bearing these considerations in mind, we shall better be able to understand the great merit of Mr. Barff's little book. His object has been to write for boys an Introduction to Chemistry which, while never rising above a matriculation standard, can condescend to touch on many of the minor and petty difficulties which such pu- pils always feel. Confining himself exclusively to this humble region, and using throughout the personal address of a teacher, he is able to enforce the matter of instruction with largely increased effect. His work is, moreover, characterized by extreme simplicity both in style and plan. The reader will not find anywhere in Part I. (206 pages) a single symbol, formula, or reference to the atomic theory. Even in Part II. (where these matters are considered) only the most direct and intelligible methods of interpretation are cau- tiously employed, it being the author's conviction that " young minds are often apt to espouse warmly views of a certain school, and to regard as facts what are nothing more than assumptions." Throughout the first part of the work the combining proportions (and minimum weights generally) are given in non-symbolic num- bers and quite dogmatically ; and it is not until a large amount of exercise in calculation has been traversed that the student is allowed to formulate anything whatever. Another excellent feature is the set of questions which occurs at the end of each chapter, and the collection of actual matriculation problems at the close of Part II. : many of these are worked out as examples. The " advice to stu- dents under examination " is also excellent.

We shall not, of course, be supposed to intimate that this book is free from defects. It is quite impossible that an effort of the kind, unique as it is, should have been unattended with some taint of the sins of modern chemistry. There is, however, quite enough in the manual to console us for such blemishes and aid us to forget them. Mr. Barff's ' Introduction ' will be of the greatest value to schools, and is a most serviceable contribution to chemical literature. The modesty and simplicity of its purpose, its freedom from theoretical partialities, and (what is not unimportant) its moderate price will ensure it a deserved and hearty commendation.

XLII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Jan. 7, 1869.—Lieut.-General Sabine, President, in the Chair.

THE following communication was read :—

"Notes of a Comparison of the Granites of Cornwall and Devonshire with those of Leinster and Mourne." By the Rev. Samuel Haughton, M.D., D.C.L., F.R.S., &c.

The granites of Mourne are eruptive, and can be proved to contain albite as their second felspar.

The granites of Leinster are also eruptive ; and although albite has never yet been actually found to occur in them, its existence can be inferred with considerable probability.

During the past summer (1868) I have succeeded in proving that the second felspar that occurs in the granites of Cornwall is albite. I found this mineral as a constituent of the granite at Trewavas Head, where it has the following composition :—

I. *Albite, var. Cleavelandite (Trewavas Head).*

Silica	65·76
Alumina	21·72
Lime	0·89
Magnesia	trace
Soda	9·23
Potash	1·76
Water	0·40
	99·76

This albite is opaque, cream-coloured, lamellar, and associated with quartz and orthoclase, which has the following composition :—

II. *Orthoclase (Trewavas Head).*

	No. 1*.	No. 2†.
Silica	63·60	63·20
Alumina	21·04	21·00
Iron and manganese oxides	trace	trace
Lime	0·90	0·68
Magnesia	trace	trace
Soda	3·08	2·75
Potash	9·91	10·30
Water	0·40	0·40
	98·93	98·33

The granites of Cornwall and Devon contain two micas, white and black. I was fortunate enough to obtain, through my friend Mr. W. J. Henwood, F.R.S., of Penzance, a sufficient quantity of white mica from Tremearne, near Trewavas Head, to determine accurately its composition, which proves to be highly interesting. It differs essen-

* From veins at foot of cliff associated with Cleavelandite albite.
† From the granite at summit of cliff.

tially from the white mica of Leinster and Donegal, and proves to be a variety of lepidolite.

III. *White Mica, Lepidolite (Tremearne, near Trewavas Head).*

Silica (SiO_3)	47·60
Fluosilicon (SiF_3)	5·68
Alumina	27·20
Iron peroxide	5·20
Manganese protoxide	1·20
Lime	0·45
Magnesia	trace
Potash	10·48
Soda	0·72
Lithia	1·14
	99·67

This lepidolite is white, pearly, and occurs in rhombic tables of 60° and 120°. Its oxygen-ratios are, reckoning for the fluorine its equivalent of oxygen :—

Oxygen-Ratios.

Silica	24·714	26·461	8·9
Fluosilicon	1·747		
Alumina	12·713	14·270	4·8
Iron peroxide	1·557		
Manganese protoxide . .	0·268		
Lime	0·127		
Magnesia	. .	2·982	1·00
Potash	1·776		
Soda	0·184		
Lithia	0·627		

This corresponds with a theoretical formula in which the oxygen of the silica is to that of the bases as 3 : 2.

The Black Mica of the Cornish granites seems to be more abundant than the White Mica already described. I found a sufficient quantity of it at Coron Bosavern, near St. Just, to enable me to make the following analysis :—

IV. *Black Mica, Lepidomelane (Coron Bosavern, near St. Just).*

Silica (SiO_3)	39·92
Fluosilicon (SiF_3)	3·04
Alumina	22·88
Iron peroxide	15·02
Iron protoxide	2·32
Manganese protoxide	1·40
Lime	0·68
Magnesia	1·07
Potash	9·76
Soda	0·99
Lithia	1·71
	98·79

The Black Mica of St. Just is of a blackish-bronze colour and metallic lustre, and occurs in rhombs of 60° and 120° angles. Its oxygen-ratios are, reckoning for the fluorine its equivalent of oxygen,—

Oxygen-Ratios.

Silica	20·727	21·645
Fluosilicon	0·918	
Alumina	10·692	15·092
Iron peroxide	4·400	
Iron protoxide	0·514	
Manganese protoxide. . . .	0·310	
Lime	0·192	
Magnesia	0·427	4·292
Potash	1·655	
Soda	0·254	
Lithia	0·940	

The oxygen-ratio of this iron-potash Mica (which is undoubtedly a lepidomelane) for silica and bases is

216 : 194, or 1 : 1.

The granites of Cornwall and Devon, which have been frequently examined by me during the last sixteen years, appear all to contain the two felspars and the two micas above analyzed. In a future communication I hope to describe their composition in detail, and to give a comparison of this composition with that of the granites of Ireland.

The following generalizations will be found, I believe, capable of proof.

(1) The granites of Ireland may be divided into two distinct classes, marked by characters both geological and mineralogical.

(2) The First Class of granites consists of Eruptive rocks, of ages varying from the Silurian to the Carboniferous periods. To this class may be referred the granites of Leinster and Mourne, and the granites of Cornwall and Devon.

(3) The first class of granites is characterized by the presence of orthoclase and albite, and by the absence of all the Lime Felspars.

(4) The Second Class of granites consists of Metamorphic rocks, of unknown geological age, but probably subsequent to the Laurentian period. To this class may be referred the granites of Donegal and Galway, and the granites of Scotland, Norway, and Sweden.

(5) The second class of granites is characterized by the presence of orthoclase and oligoclase, or Labradorite, or some other of the Lime Felspars, and by the absence of albite.

[Continued from p. 156.]

Nov. 11th, 1868.—Prof. T. H. Huxley, LL.D., F.R.S.,
President, in the Chair.

The following communications were read:—

1. "Note comparing the Geological Structure of North-western
Siberia with that of Russia in Europe." By Sir R. I. Murchison,
Bart., K.C.B., G.C.St.S., F.R.S., V.P.G.S., &c.

Count A. von Keyserling had communicated to the author the
following facts :—The district between the rivers Lena and Jenissei
is occupied by Upper Silurian rocks of the same type as those found
in the region of Petchora, and by Carboniferous rocks containing
seams of coal. The chief Secondary deposits are of Oolitic or
Liassic age, and agree with those of the Petchora region, which is
the next adjacent tract on the west to the Siberian region in ques-
tion. Similar rocks are found in Spitzbergen. The banks of the
Jenissei are covered with Postpliocene accumulations similar to
those found near Archangel. It is thus seen that the vast, slightly
undulating, and to a great extent horizontal and unbroken forma-
tions, each of which occupies so wide an area in European Russia,
are repeated on the eastern side of the Ural Mountains. In this
range of mountains only are to be found igneous and erupted rocks.

In conclusion, Sir Roderick referred to the discovery of fossili-
ferous white chalk in parts of the great Sarmatian plain by M.
Grewinck.

Sir RODERICK MURCHISON, in explanation of the paper, referred to
a geological map of Russia, and gave a general sketch of the bearing
of the paper on the previously known geology of that country. He
mentioned the discovery by M. Grewinck of beds of brown coal
containing amber, and overlying true chalk. The amber in the
Baltic had been supposed to have been washed out of beds beneath
the sea ; but Count Keyserling has suggested that the amber may
have been brought down by the rivers from the interior, and depo-
sited in the Baltic. Sir Roderick also called attention to the ab-
sence of igneous rocks in Russia to the west of the Ural Mountains.

2. "On a Section of a Well at Kissingen." By Prof. Sand-
berger, For.Corr.G.S.

Taking as a starting-point a bed of dark-blue limestone, the
author proceeded to describe the various beds passed through in
sinking the Schönbern well, both as regards their petrological cha-
racters and chemical constitution. He considered that this bed is
on the same horizon as the uppermost Plattendolomite of the Zech-
stein formation in the Harz and Thuringia. Above this lie the
lowermost beds of the Bunter (containing dolomites), and below it
the upper part of the Zechstein formation. Below the Plattendolo-

mite of the Zechstein, from the depth of 1740 feet to 1884 feet, follow the saliferous beds.

3. "On the Formation of Deltas; and on the Evidence and Cause of great Changes in the Sea-level during the Glacial Period." By Alfred Tylor, Esq., F.L.S., F.G.S., &c.

The first portion of this paper was devoted to a comparison of the delta-deposits of the Po, Ganges, and Mississippi. The surfaces of these deltas and the alluvial plains above them were compared together; and it was stated that a parabolic curve drawn through the extremities of each river, and through one point in its course, nearly represents its longitudinal section—the greatest deviation being 30 feet in some of the largest deltas.

The littoral deposits around Great Britain described by Mr. Godwin-Austen were next investigated, to ascertain whether the hypothesis of a fall of 600 feet in the sea-level is tenable. The ice-cap at the poles was also alluded to as a probable cause of a great reduction of the sea-level during the Glacial period.

The upper 600 feet of deposits in the Pacific Ocean, made by coral-zoophytes, were quoted as cases which might be explained as well by oscillations in the sea-level as by the received hypothesis of the subsidence of the sea-bottom.

Prof. E. Forbes's investigations into the origin of the fauna and flora of the British Isles were next alluded to, and the author considered that the hypothesis of a fall in the sea-level better accords with the facts of migration than Forbes's suggestion of changes of the level of the land and sea-bottom.

The origin and age of the English Channel was discussed at some length; and the occurrence of the Crag and fossiliferous gravels and raised beaches near the same level, although of different ages, together with the evidence afforded by the dredging up of fresh-water and littoral shells in the North Sea and English Channel, were adduced in support of the theory of the depression of the sea-level.

The parabolic curve not only represents the curve of deposition; for the author had measured other sections, and found that the curves of denudation and deposition approximate often to that of the parabola.

Discussion.

The PRESIDENT called attention to the fact that in the neighbourhood of coral reefs the dead corals extend to such a vast depth that, supposing them all to have been formed near the surface, and that surface only lowered by abstraction of water to the Poles, the accumulation of ice must have been so great as to become incredible.

Sir CHARLES LYELL had already suggested to Mr. Croll that, assuming the accumulation of ice at the Pole depressing the centre of gravity of the earth, the submergence that would have resulted had

the quantity of water in the sea remained the same would, to some extent, be counteracted by the reduction in volume consequent on the formation of the ice. With regard to the delta of the Missis- sippi, the data on which he argued had considerably altered since first he wrote on the subject, inasmuch as recent calculations had doubled the estimated volume of water flowing into the sea, and thus it was capable of producing the same effect in half the pre- viously calculated time. The progress of the delta at any spot was of necessity variable, as the position of the mouth changed. The American engineers had allowed only 40 feet as the depth of the fluviatile deposits, whereas from boring Sir Charles had concluded it to be at least 500 or 600 feet. There was now reason to suppose that it was much more, possibly as much as 1500 feet. This being the case, notwithstanding the amount of work done by the river being doubled, his calculation as to the time required for the formation of the delta might not after all be so excessive.

Mr. Prestwich suggested that Mr. Croll's theory only involved a transfer of ice from one Pole to the other, and not a diminution of volume of the sea. The raised beaches round the coast of Britain varied considerably, and were not on one uniform horizon, as they would have been had they resulted from a lowering of the sea. The elevation of the old sea-beds during the Glacial period were not accounted for by any supposition of the mere alteration in the volume of the sea.

Mr. Evans pointed out that, the *Cyrena* being a freshwater shell, its position at a certain level was not connected directly with the height of the sea. He doubted the curve of the rivers being in all cases parabolic.

Mr. Mallet had already remarked that the beds of rivers, especially near their sources, appeared to assume curves closely allied to a parabola. He considered that the form was due rather to the elevatory forces than to erosion. He doubted, however, whether they were really parabolic curves, or indeed any other ma- thematical curve.

Mr. Tylor replied that he had not found definite evidence as to the extension of corals downwards to such a depth as that men- tioned by the President. With regard to oscillation, he had merely treated of the southern part of England. The opening of the Straits of Dover would account for the existence of beaches above the pre- sent level, as the tides would have previously risen higher. The parabolic curve was that which, by actual comparison, coincided most closely with the longitudinal section of the banks of the rivers Po, Mississippi, and Ganges.

ON THE COLORATION OF PEROXIDE OF NITROGEN.
BY M. SALET.

THE vapour of peroxide of nitrogen (hyponitric acid) has several remarkable properties. Its density rapidly decreases up to $43°$; the decrease then becomes slower, and it ceases at $150°$*. It is at the same time observed that the colour of the vapour gradually deepens; from yellowish brown it becomes reddish brown and then deep red.

It may be asked whether the same cause cannot explain these two peculiarities.

Wurtz supposes that the molecule of peroxide of nitrogen at a low temperature contains $N^2O^4 = 2$ vols., and that on being heated it is gradually dissociated into 2 molecules, NO^2, each occupying two volumes (*Chimie Moderne*, p. 156). This decomposition, which is the inverse of polymerization, is not without examples in chemistry; the cases of cyanic acid, styrolene, &c. may be adduced.

Supposing that NO^2 and N^2O^4 expand regularly, it is easy to calculate the composition of a mixture of these two bodies which for a given temperature would have the density found for peroxide of nitrogen.

Let D be this density referred to hydrogen (that is, the weight of the volume of peroxide of nitrogen equal to that of H); let a and b be the quantities in weight of NO^2 and N^2O^4 contained in this volume. As we have $\frac{1}{2}(NO^2) = 23$ and $\frac{1}{2}(N^2O^4) = 46$ for the theoretical densities of NO^2 and N^2O^4 referred to hydrogen, the following relations may be established:

$$a + b = D, \quad \frac{a}{23} + \frac{b}{46} = 1;$$

whence

$$a = D - 46, \quad b = 2D - 46.$$

Table A gives the values of $\frac{a}{D}$; that is, the proportion in weight of NO^2 calculated from the experimental densities determined with so much accuracy by MM. St.-Claire Deville and Troost†. It will be observed that these latter numbers are a little too high, corresponding to an experimental density a little weaker than the theoretical.

This being granted, we may reason as follows:—Since peroxide of nitrogen is colourless at a temperature at which its density probably corresponds to N^2O^4, and its colour is deeper the nearer we approach the temperature at which the molecular condensation corresponds to the formula NO^2, let us assume that N^2O^4 is colourless and NO^2 coloured, and investigate the consequences of this hypothesis.

* MM. H. St.-Claire Deville and Troost.
† *Comptes Rendus*, vol. lxiv. p. 237.

Table A will serve to construct Table B; this latter gives the length x which a column of nitrous vapour at $26°{\cdot}7$ must have, on our hypothesis, to present the same degree of colour as a column of vapour of constant length and equal to unity but of varying temperature. It is calculated by the formula

$$x = \frac{a}{D} \frac{P}{3 \cdot 1214 \times 20 \cdot 26},$$

in which P is the weight of the unit of volume of peroxide of nitrogen at t degrees. It will be seen from an inspection of this Table that the coloration at first rapidly increases with the temperature—that it then attains a maximum, because the increase in specific coloration is balanced by a decrease in density—that, finally, the effect of this decrease predominates, so that the coloration itself decreases indefinitely.

Tempera-ture, t.	A.		B.	
	Weight of a litre of vapour, P.	Proportion in weight of N O², $\dfrac{a}{D}$.	Theoretical coloration, $\dfrac{a}{D}\dfrac{P}{3\cdot1214\times20\cdot26}$.	Experimental numbers.
$2\overset{\circ}{6}{\cdot}7$	3·1214	20·26 per cent.	1	1
35·4	2·8975	25·8 ,,	1·182	1·18
39·8	2·7745	29·6 ,,	1·299	1·28
49·6	2·4793	40·5 ,,	1·588	
60·2	2·1980	53·3 ,,	1·852	1·9
70·0	1·9768	66·1 ,,	2·066	
80·6	1·7973	76·9 ,,	2·185	2·2
90·0	1·6744	85·1 ,,	2·253	
100·1	1·5892	89·7 ,,	2·254	2·3
111·3	1·5144	93·3 ,,	2·234	2·25
121·5	1·4519	96·6 ,,	2·218	2·24
135·0	1·3814	99·1 ,,	2·165	2·2
154·0	1·3082	101·7 ,,	2·104	2·12
200	......	....	1·9	1·95
225	......	....	1·8	
250	......	....	1·7	
275	......	....	1·6	1·6
300	......	....	1·56	1·52

These consequences of our hypothesis have been experimentally verified. By means of a special calorimeter we determined the values of x with a sufficient degree of approximation; the means of the experiments are met with in the last column of Table B. We have carried the experiment beyond the temperatures for which the vapour-density has been determined by MM. Deville and Troost; we have assumed with these chemists that the density is then normal.

Our apparatus consists of two prisms for total reflection, which send the light of the zenith through two horizontal tubes closed by glass plates and placed in the same right line. These tubes contain vapours under the ordinary atmospheric pressure; they are heated

in an air-bath; and one of them may be lengthened or shortened at pleasure. In the space between them two other prisms for total reflection are placed, which send the two coloured beams parallel, so as to form two tangential images as in the saccharometer. The tube of fixed length is gradually heated; and equality in colour is obtained by varying the length of the second tube, which is kept at a temperature of 26°·7.

It was previously ascertained that the principle of compensation adopted was applicable; for two columns of vapour, the one hot and short, and the other cold and long but presenting the same colour to the eye, gave the same spectrum when the same image was examined by a direct-vision spectroscope the refracting edges of which were horizontal.

Spectroscopic examination showed that the colour of a column of nitrous vapour cannot be compensated by that of a column of liquid peroxide of nitrogen. The spectrum of this latter does not present bands of absorption, only a maximum of intensity in the reddish yellow.—*Comptes Rendus*, August 8, 1868.

ON THE MAGNETISM OF CHEMICAL COMPOUNDS.
BY PROFESSOR WIEDEMANN.

The magnetic deportment of chemical compounds of magnetic metals is highly interesting, inasmuch as the metals in them frequently retain to a greater or less extent the magnetism which they possess in the free state, and thus, by a determination of this magnetism, conclusions may frequently be drawn in reference to the properties of the metals themselves in their combinations. From this point of view the author had determined in a former investigation the degrees of magnetism of various oxygen and haloid salts of the magnetic metals, and had found *that in analogous salts of the same metal the product of the atomic weight with the temporary magnetism excited by the unit of magnetizing force in the unit of weight of the salt (that is, the magnetism of an atom of these salts) is almost constant.* If this atomic magnetism for ferric-oxide salts is 466, its mean value for chromic-oxide salts is 190·8, and for protosalts of manganese, iron, cobalt, and nickel the numbers respectively 468, 307, 313, and 142.

I. Recent investigations, made by an entirely analogous method to the former, have shown that the same deportment also prevails in the oxygen and haloid salts of cerium, didymium, and copper. Retaining the former unit, we have for the atomic magnetism in aqueous solutions the numbers—

Didymium sulphate	....	104·4
,,	nitrate..........	104·2
,,	acetate..........	105·2
,,	chloride	105·7

Cerous nitrate	....	48·7
Cerium chloride	....	47·6

Cupric sulphate	....		49·5
,,	nitrate	.. ..	50·7
,,	chloride	....	48·9
,,	bromide	....	47·7
,,	acetate	....	48·0

II. In the solid salts, too, almost the same values are obtained, especially when they contain water of crystallization. Thus the atomic magnetism for

Crystallized didymium sulphate is ... 107·2
Crystallized cupric sulphate is ...... 49·6

If, however, the solid salts are anhydrous, their atomic magnetism is in general somewhat less, as has been found in a previous investigation in the case of cobaltous sulphate, ferrous oxide, and ferrous chloride.

Thus the atomic magnetism of anhydrous cerous sulphate is 44·9.

These diminutions in the atomic magnetism of anhydrous copper salts are considerable, even when they are combined with ammonia. It is, for

Anhydrous cupric sulphate		....	42·2
$2\,CuSO^4 + 5\,NH^3$		....	43·5
$2\,CuSO^4 + NH^3$		....	45·1
$2\,CuSO^4 + 2\,NH^3 + aq$		....	42·1
$2\,CuSO^4 + NH^3$		....	44·6
Cupric chloride (anhydrous)		....	40·0
,,	,,	saturated with ammonia.. :	37·1
,,	,,	dissolved in water	47·2
,,	bromide (anhydrous)	....	24·0
,,	,,	saturated with ammonia....	41·1
,,	,,	dissolved in water	48·0

A similar deportment is exhibited by the solid nickel salts. While the mean atomic magnetism of the dissolved salts is 142, that of

Crystallized nickel sulphate is .... 139·2
Anhydrous nickel sulphate........ 131·0
Ammonium nickel sulphate ...... 135·6

The solid anhydrous chlorides of nickel and of cobalt exhibit a higher atomic magnetism (156 and 378–395 respectively).

These variations obviously depend upon the different densities, and are especially prominent with very dense salts—for example, cupric bromide (specific gravity $=4\cdot38$). If the connexion between the particles of the salts is diminished by the intervention of water, as in the case of the hydrates or of ammonia (as in the ammoniacal copper salts), the atomic magnetism s at once diminished, and still more so by dissolving the salts.

III. The magnetism of the copper salts is very remarkable, and

particularly that of cupric bromide, a salt both whose constituents (copper and bromine) are feebly but decidedly diamagnetic, as direct determinations show. *Two diamagnetic elements can thus by their combination give a magnetic compound.*

That this magnetism of copper salts is to be ascribed to the copper itself is proved by its constancy in various salts, even when, as in the double cyanides, the magnetism of the simple or compound radical united with it is changed.

IV. In other investigations two solutions of known magnetism, M_1 and M_2, which change their constituents by double decomposition, were mixed in a glass and the magnetism M_m of the mixture investigated.   Among others the following solutions were mixed :—

			$M_1 + M_2$.	M_m.
Ferric chloride and potassium ferrocyanide		....	20·4	21·1
Ferrous sulphate	,,	,,	.... 41·2	40·3
Cupric sulphate	,,	,,	.... 0·7	0·8
Nickel sulphate	,,	,,	.... 20·3	22·5
Ferric chloride and potassium sulphocyanide		..	15·1	14·6
Manganous sulphate and potassium ferrocyanide.			71·8	70·2

The solid form in which one of the salts formed by double decomposition is frequently precipitated exercises in only a few cases any disturbing influence.

Hence, if we mix two salts whose constituents change by double elective affinity, the total magnetism of the solutions is the same after double decomposition as before.

We can draw from this with great probability the following conclusion :—*That the magnetism of a binary compound is made up by simple addition of the magnetisms of both its constituents in their respective conditions ; and that these constituents, when they enter into other binary compounds without changing their constitution or atomic grouping, retain their atomic magnetism without alteration.*

V. From the equality of the atomic magnetism of solid *potassium ferrous oxalate* with the atomic magnetism of other ferrous salts, as well as of *potassium ferric oxalate* and of *potassium iron alum* in a solid form with that of the other ferric salts, we can prove, in opposition to the views based on the peculiar colour of the salts *, that in these salts in the solid form the iron is contained in the same manner as in other ferrous and ferric salts.   The constancy of the atomic magnetism proves that in the different coloured chromic salts the magnetic atomic group retains its properties unchanged.   It also remains unchanged if *anhydrous copper or nickel salts combine with water*, an observation which does not agree well with Graham's views on the constitution of the ammoniacal copper salts.   On the other hand, *luteocobaltic chloride* and *purpureocobaltic chloride* are diamagnetic ; so that, unlike the copper salts, they are not to be regarded as simple cobaltic salts united with ammonia ; the atomic group containing the metal and determining the atomic magnetism must be essentially different from what it is in the simple salts.

* Compare Haidinger. Pogg. *Ann.* vol. xciv. p. 246.

VI. If u_s stand for the atomic magnetism of the dissolved salts of the magnetic metals, the atomic magnetism u_h of their hydrated oxides freshly precipitated from their solutions is as follows:—

Hydrated nickel monoxide	$u_h =$	$1 \cdot 00\,u_s$
,, cobalt monoxide		$1 \cdot 12$
,, ferrous oxide.		$1 \cdot 12$
,, manganous oxide		$0 \cdot 85$
,, cupric oxide		$0 \cdot 74$
,, chromic oxide		$0 \cdot 95$
,, ferric oxide		$0 \cdot 69 – 1 \cdot 18$

Thus the atomic magnetism of the hydrates of the magnetic metals is partly equal to, and partly smaller or somewhat larger than that of the corresponding salts; so that, having regard to the influence of density, it may be assumed that the group which determines the magnetism in them has the same constitution as in the corresponding salts.

VII. On *precipitating ferric hydrate* from its salts, the atomic magnetism of the precipitated oxide rapidly rises from $0 \cdot 67$ to $1 \cdot 129\,u_s$; this is probably due to the circumstance that in the first few moments the ferric hydrate is still partially dissolved in the colloidal condition.

The colloidal dissolved ferric oxide has an atomic magnetism ($100 \cdot 2$) which is only about $0 \cdot 2$ that of the ferric salts. In like manner the feeble atomic magnetism of ferric acetate under different circumstances (114 to 147) shows that in this case also a great part of the ferric oxide is dissolved in the colloidal state. In any case the magnetic atomic group in this colloid ferric oxide is different from what it is in the precipitated ferric hydrate and in the ferric salts.

This difference is not met with in the chromic oxide dissolved in potash, where the chromium both before and after coagulation has the same atomic magnetism as in the salts. In like manner the atomic magnetism of *nickel monoxide dissolved in ammonia* is the same as that of the hydrate.

VIII. The magnetism of the oxides of the magnetic metals is far less than that of the salts; yet it is different according to the mode of preparation and the density. The atomic magnetism was found to be, for

Freshly ignited nickel monoxide			$66 \cdot 4$
,, ,, didymic oxide			$55 \cdot 0$
,, ,, cupric oxide			$10 \cdot 5$
,, ,, manganous oxide			$144 \cdot 0$
,, ,, chromic oxide			66
,, ,, chromic oxide precipitated along with alumina			107
,, ,, ferric oxide			59
,, ,, ferric oxide precipitated along with alumina			256

It cannot yet be determined whether the magnetism of the oxides,

which is so small in comparison with the magnetism of the hydrates, depends on an alteration in the density of the whole mass, or only on an alteration of the magnetic atomic group itself.

IX. The *sulphur compounds* corresponding to the salts of the magnetic metals investigated are, with the exception of sulphide of manganese, very feebly magnetic.

X. Nickel cyanide and cobalt cyanide have an atomic magnetism which is only about 0·4 to 0·6 of the atomic magnetism of the other salts of nickel and cobalt. When these cyanides are dissolved in solution of cyanide of potassium, their magnetism almost entirely disappears. This cannot arise from the formation of a simple double salt ; for the magnetic constituents in the double salts retain their atomic magnetism unchanged ; the magnetic atomic group must rather have itself changed. The salts formed have probably the composition $2\,KCy$, $CoCy^2$, and $2\,KCy$, $NiCy^2$, corresponding to their electrolytic deportment. The analogous magnetic deportment of potassium ferrocyanide and potassium ferridcyanide favours this view. From the experiments on the decomposition of magnetic salts by double elective affinity, the potassium can be replaced by the magnetic metals, which thereby retain their atomic magnetism unchanged as in the ordinary oxygen and haloid salts. From the analogy with the latter they would also have to be regarded as consisting of an equivalent of potassium combined in potassium ferrocyanide with a diamagnetic atomic group $(Cy + \frac{1}{2}\,FeCy)$, by which the salt itself is diamagnetic, and in potassium ferridcyanide with a magnetic atomic group $(Cy + Fe\frac{2}{3}\,Cy)$, by the addition of which the salt becomes magnetic.

XI. The *atomic magnetism of the three salts of manganese, iron, and cobalt corresponding to potassium ferridcyanide* is, both when investigated in the solid and in the liquid state, for one equivalent of the magnetic metal (for instance $Fe = 28$, &c.),—

<div align="center">

Potassium manganicyanide.......... 145·4

,.   ferrocyanide .... 73·9

,,   cobalticyanide............ 3·5

</div>

As in the oxygen and haloid salts of the three metals, *the atomic magnetism of potassium ferrocyanide is the mean between that of potassium manganicyanide and potassium cobalticyanide*; and the three atomic magnetisms of these salts are less by almost an equal amount than the magnetisms of the oxide salts of the same metal, as if a strong diamagnetic atomic group had been joined to the magnetic metals in them. In the sulphocyanides the metal has the same magnetic properties as in the simple salt of the same metal.—*Berliner Monatsberichte,* July 1868.

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ON THE LATENT HEAT OF VOLATILIZATION OF SAL-AMMONIAC.
BY M. C. MARIGNAC.

Sal-ammoniac when volatilized occupies double the volume required by a theory which of late years has had numerous partisans. Is this to be ascribed to an anomaly in the physical constitution of

its vapour, or should it be explained by the dissociation of the elements?

We owe to M. H. St.-Claire Deville an ingenious experiment which proves that in all cases the decomposition of sal-ammoniac is not complete. Ammoniacal gas and hydrochloric acid gas heated to 360°, and brought together in a space kept at this temperature, would manifest their combination by a disengagement of heat which would raise the thermometer beyond 390°. It is therefore impossible to assume that in the vapour of sal-ammoniac at 360° the elements are quite in a state of liberty. But it is not thereby proved that at this temperature there is not a partial decomposition, which may reach an amount sufficient both to explain the disengagement of heat arising from the mixture of the two gases, and the low density of sal-ammoniac.

Among the experiments which might throw some light on this point, if not solve it, one of the most important would be a determination of the latent heat of sal-ammoniac. For if this volatilization is due to a simple change of state, it ought only to absorb a quantity of heat analogous to that required to produce this same physical modification in other compounds. If, on the contrary, it is accompanied by a more or less complete chemical decomposition, it should require a far greater quantity of heat, not greatly different from that which results from the chemical combination of ammoniacal gas and hydrochloric acid.

These considerations have led me to attempt this determination; but it presents such difficulties that I can only offer my results as a rough approximation, sufficient, however, for the object in view.

The method usually employed for the determination of the latent heats of vapours is inapplicable in the present case; for it is impossible to transport the vapour of sal-ammoniac from the vessel where it is formed to the calorimeter. When it is no longer in contact with surfaces heated to 350°, it condenses and soon stops up even the widest tubes.

I have endeavoured to invert the problem, and to measure the quantity of heat used in volatilizing the salt in the open air, as compared with that required for the volatilization of water under the same circumstances.

The apparatus I used consists of a massive cast-iron cylinder in which three cavities are perforated symmetrically about the axis; in one of these is an air-thermometer, in the two others the substance to be volatilized.

The cylinder heated to redness is transferred to a box with badly conducting sides, in such a manner that its upper face is exposed to the air.

The substance to be volatilized, contained in thin glass or silver tubes, is placed in the cavities of the cylinder the moment it attains a given temperature (500° for instance). The tubes are withdrawn when the thermometer indicates 420°; the loss of weight they have experienced gives the amount volatilized.

On the other hand, an investigation of the cooling of the apparatus, made by numerous experiments both when it contained no sub-

stance in the interior and when part of its heat was used in volati-
lizing either water or some volatile substance, enables us to calculate,
not exactly, perhaps, but at all events with sufficient approximation,
the quantity of heat consumed in each case.

For the details of the method I must refer to the memoir which I
shall publish on the subject, and I limit myself here to indicating the
principle of the method.

I find that the heat of volatilization of 1 gramme of sal-ammoniac
is 706 thermal units, with a great probability that its real value is
between the limits 617 and 618.

The magnitude of this number, compared with that of the various
compounds for which it is known, and, on the other hand, its agree-
ment with that for the heat of combination of ammoniacal and hy-
drochloric acid gases*, make it highly probable that sal-ammoniac is
indeed partially decomposed into its elements when it is vaporized.

To strengthen this conclusion, and to be certain that the high
numbers are not solely due to the imperfection of the method used,
I have endeavoured to apply it to determining the latent heats of
some other substances ; but having only made a few experiments
in each case, I shall cite the results without attaching any other im-
portance than that of showing that my method does not necessarily
lead to high results in the case of bodies which approach sal-ammo-
niac in their physical properties (boiling-point or solid state).

Mercury, 103 to 106.—This must be regarded as a minimum ;
for a considerable quantity of mercury condenses at the orifice of the
tubes and falls in the interior in droplets.

Mercurous chloride, 72 to 131, according as we do not, or do,
allow for the portion of salt vaporized in the tubes, but condensed
at their orifice. This difficulty does not occur in the case of sal-am-
moniac, the fumes of which, being very light, are easily carried
away by the motion of the air.

Mercuric chloride, 28 to 45.—These numbers comprise both the
latent heat of fusion and that of volatilization.

Monohydrated sulphuric acid, 292 to 342.—These high numbers
seem to justify the hypothesis of the dissociation of this acid as-
sumed by Messrs. Wanklyn and Robinson. It may, moreover, be
remarked that this heat of volatilization would exactly agree with
the heat of combination of anhydrous sulphuric acid and water.

I must, in conclusion, remark that when I speak of the dissocia-
tion of sal-ammoniac or of sulphuric acid, I attach to this term the
meaning which M. St.-Claire Deville has given to it—that of a par-
tial decomposition, the resultant of a state of equilibrium, varying
with the temperature, between the elements of a body and the com-
pound they tend to form. The principle of my experiments, and
the uncertainty which prevails as to their results, do not justify us
in concluding that there is a complete decomposition, which, more-
over, seems impossible within the limits of temperature attained in
my experiments.—*Comptes Rendus,* November 2, 1868.

* This heat of combination, according to MM. Favre and Silbermann, is
743·5 thermal units at the ordinary temperature ; it would be 715 at a
temperature of 350°.

XLIV. *Researches in British Mineralogy.*
By DAVID FORBES, *F.R.S. &c.**

III.

Native Gold.

IN the first part of these researches, which appeared in 1867 in the November Number of this Magazine, I communicated the results of my investigations into the chemical composition and geological occurrence of the native gold which is met with in the quartz lodes and alluvial river-deposits of North Wales. Since that time the recent discoveries of gold in the north of Scotland have attracted much attention, and rendered the subject of the occurrence and distribution of the noble metal in the British Isles one of somewhat more than ordinary interest—a circumstance which has induced me to extend this inquiry into an examination of the native gold from all the British localities whence I am able to procure authentic specimens.

Although it will be found that, even from the most ancient periods in English history, statements of discoveries of gold have been from time to time recorded as having been made in numerous places throughout England from north to south, none of them seem to have at any time led to a practical or successful result; and certainly the majority of such accounts do not upon scrutiny appear to be entitled to any confidence being placed either in their correctness or veracity; in fact the only districts in England where unquestionable evidence of the presence of native gold can be obtained are Devonshire and Cornwall.

* Communicated by the Author.

It must be remembered, however, that gold in minute quantity is undoubtedly contained in some of the English lead and copper ores, especially when these ores are strongly argentiferous, as will be hereafter noticed. Still the total amount of gold present in such cases is so extremely insignificant as neither to entitle them to the appellation of gold ores proper, nor even, in most if not all cases, sensibly to augment the value of the ore itself, since, as a rule, its value would scarcely be found to cover the expense of its extraction or separation from the other metals with which it is associated in the ore as it comes from the mine.

In Devonshire the Poltimore mines at North Molton are traditionally reported to have been worked for gold by the Romans during their occupation of Britain; and in 1853 the brown and red " gossans " forming the substance of this lode at its surface-outcrop or " backs," as it is called by the miners, were treated as gold ores, and stated to yield between one and two ounces of fine gold per ton of " gossan." Still, although a not inconsiderable amount of gold was extracted from them, the final result of the adventure did not prove remunerative. The chemical examination of these " gossans," made by myself in 1854, indicated that they were a product of the decomposition of slightly auriferous metallic sulphides (principally iron), due to oxidation and other atmospheric influences; and whilst it proved that these gossans did contain a small amount of gold, it at the same time showed that the actual amount present in them was much less, and much more irregularly disseminated throughout the mass, than had generally been reported.

Native gold along with stream-tin ore (cassiterite) has also been washed out of alluvial deposits at Shepstor in Dartmoor, which have no doubt been formed from the débris arising from the disintegration of the granitic rocks of that district.

Native Gold from Cornwall.

In this district the occurrence of gold *in situ* in its original rock-matrix is extremely rare; but it is reported to have been found in a cross course in Huel Sparnon, and the gossan of the Nangiles mine is said to be auriferous. In the British Museum a specimen of gold in quartz along with argillaceous or possibly chloritic slate is exhibited; but as no locality is given besides Cornwall, I am uncertain as to whether it is to be regarded as thoroughly authentic or not.

In a recent visit to the north of Cornwall I found that the argentiferous tetrahedrite (Freibergite), chalcopyrite, and galena from a lode at Bound's Cliff near St. Teath, contained a minute but distinct amount of gold; but I have as yet not been able to determine its amount quantitatively.

It has long been known, however, that the alluvial deposits in the beds of many of the Cornish rivers contain native gold in the form of nuggets and small rounded or flattened grains or scales; and it appears not improbable that the gold forming the ornaments of prehistoric origin frequently found in Cornwall and the neighbouring counties may have been derived from this source. A nugget of gold from these alluvial deposits in the collection of the late Sir Charles Lemon weighs more than one ounce, and the largest nugget recorded is stated to have weighed 2 ozs. 3 dwts.; but these are quite exceptional instances, as in general the weight of the particles does not exceed a few grains.

Most frequently the gold is found associated with the so-called stream-tin (cassiterite or oxide of tin); and when the stream-works were, as anciently, an important source of the tin-production of Cornwall, a not inconsiderable quantity of native gold seems to have been obtained during the washing of the alluvial deposits for tin ore. As these deposits have now been nearly, if not altogether, exhausted, it has become extremely difficult to meet with authentic specimens of Cornish gold at the present time.

In the British Museum may be seen a specimen of stream-tin ore with gold from the Carnon stream-works; and Messrs. Greg and Lettsom mention gold being found at the Crow Hill stream-works at Trewarda, Kenwyn, at Llanlivery near Lostwithiel, and at Laddock near Grampound.

As no analysis of native gold from Cornwall is to be met with in any of the works of mineralogy which I have examined, I have for several years been endeavouring to secure an authentic specimen for the purpose of determining its chemical composition, but only succeeded last month, when Mr. W. J. Henwood, F.R.S., kindly sent me a specimen, obtained through Mr. Petherwick of St. Austell, from St. Austell Moor.

The gold thus obtained was of a rather deep gold-colour, but of a dingy lustre, the surface of the particles being worn and rounded by attrition; it consisted of nine minute nuggets of very irregular forms, the largest of which weighed 2·1 grains, whilst their aggregate weight was only 5·46 grains.

The specific gravity, determined upon the entire quantity at disposal, was found to be 16·52 at 60° Fahr.; and the chemical analysis gave the following numbers as the percentage composition of the gold itself:—

Gold	90·12
Silver	9·05
Silica with sesquioxide of iron . .	0·83
	100·00

results which closely approximate to the composition of the gold from the Clogau mine in North Wales previously examined by me, and which correspond to the formula $Au^6 Ag$, which by calculation requires 90·88 per cent. gold to 9·12 per cent. silver.

The geological age of the appearance of this gold is, I believe, identical with that of the intrusion of the stanniferous and auriferous granites of Cornwall, which appears to be some time between the Silurian and Carboniferous periods; and it seems not improbable that the greater portion of both, the stream-tin ore as well as the native gold, was originally disseminated throughout the mass of the granite itself, and subsequently, through its disintegration, became set free and washed down into the alluvial deposits formed by the rivers.

Native Gold, Wicklow, Ireland.

To the good offices of Mr. F. Jennings I am indebted for two small nuggets of gold from the Wicklow alluvial deposits, which have enabled me to determine their composition.

The specimens were rounded irregular masses, and evidently contained internal cavities, so that the determination of their specific gravity is not to be relied upon.

Their colour was a rather brassy golden hue, whilst the external surface of the gold was abraded and possessed a very dingy lustre.

The specific gravity of the one nugget was found to be 15·07, whilst that of the other was only 14·34—probably owing to the existence of internal cavities or quartz particles.

The analysis was conducted upon the first-mentioned of these nuggets (spec. grav. 15·07), and in precisely the same manner as described in the case of the gold from the Clogau mine in the first part of these researches.

The results obtained gave the following percentage composition :—

Gold	.	.	.	.	91·01
Silver	.	.	.	.	8·85
Quartz	.	.	.	.	0·14
					100·00

These results do not coincide with the analysis of Mr. W. Mallet, published in the Journal of the Geological Society of Dublin, vol. iv. p. 271, but are almost identical with those obtained by Mr. R. Scott, published in Sir W. Wilde's ' Catalogue of the Gold Antiquities in the Collection of the Royal Irish Academy,' 1862, when the amount of iron obtained by him is subtracted*.

* Mr. Scott informs me that this was probably due to entangled pyrites, and that he also found a trace of copper present.

The results of these two analyses are given as follows :—

	Mallet.	Scott.
Gold	92·32	89·0
Silver . . .	6·17	8·1
Iron	0·78	2·1
	99·27	99·2
Specific gravity .	16·342	?

Several large nuggets of the gold from the washings on the river on the north-east side of the mountain Croghan Kinshela in the county of Wicklow, on the borders of Wexford, are to be seen in the British Museum ; and the largest nugget known to have been obtained is stated to have weighed twenty-two ounces.

The minerals associated with this gold are as follows :—

Cassiterite (tinstone), magnetite, hæmatite, pyrite, titanoferrite, wolfram, wad, chalcopyrite, galena, molybdenite, garnet, chlorite, felspar, and mica—as well as, according to Mr. Mallet, topaz, sapphire, zircon, and native platinum.

This gold is, without doubt, derived from the disintegration of the granite of the district, which appears to be what I have elsewhere described as a normal auriferous granite composed of orthoclase, quartz, and mica, similar to the Cornish rock, and, like it, both stanniferous and auriferous.

That the gold along with the cassiterite and wolfram really exists as a constituent of the granite itself, or, in other words, is disseminated throughout its mass, is in accordance with the conclusions arrived at by Mr. Weaver, who, with Messrs. Mills and King, was appointed Director of the Government gold-washings on the Ballinvally stream at Croghan Kinshela in 1796. The observations of Mr. Weaver are contained in the report on the geological relations of the east of Ireland, published in the Transactions of the Geological Society, vol. v. p. 211 ; and he mentions therein that the total gold obtained during the period that the washings were worked on account of the Government amounted to 944 ozs. 4 dwts. 15 grs., or in value £3675 7s. 11½d., and that up to the time of the Irish Rebellion in May 1798 the gold obtained had not only fully reimbursed the advances made by the Government, but had, besides defraying its expenses, left a surplus in hand.

Native Gold, Sutherlandshire.

In the sixteenth century gold is said to have been discovered in some quantity, and worked, at Durness on the north coast of Sutherlandshire ; and in 1840 a solitary nugget of more than one ounce in weight is reported to have been found in the river of

Kildonan. It was only, however, towards the close of last year that the localities which now are being somewhat extensively explored were discovered by a Mr. Gilchrist, a native of Sutherlandshire, who had but recently returned from Australia, where he had been engaged in the gold-diggings for a number of years. The consequent rush of adventurers to these auriferous deposits has led to finding gold in the beds of many of the neighbouring streams; but as yet no authentic information has been received as to the occurrence of gold *in situ* in the rocks of this district.

The first find was made in the Kildonan Strath, about ten miles inland from Helmsdale, and a little later at Siesgill Burn, about two miles above Kildonan. At the commencement of this year a further discovery of gold was made in the Strath of Dunbeath in Caithness-shire, about the end of January, in two different burns between Ben Smesral and Gordonbush, and subsequently in Auldtoun Burn and at Kilcomkill (about six miles from Brora), in a tributary of the river Brora.

Mr. James Haswell of Edinburgh kindly put me in communication with Mr. P. G. Wilson* of Inverness, who has furnished me with specimens of the gold produced from the Kildonan washings, and thus enabled me to submit them to a chemical examination.

The gold from the Kildonan diggings, as they are called, which I received from Mr. Wilson, was in the form of minute grains more or less flattened, and varying in size from the smallest speck up to that of a split pea, the largest of them weighing about 5 grains. I understand, however, that lumps of much greater size have been occasionally met with, and, amongst others, that the original discoverer (Mr. Gilchrist) had in his possession five nuggets varying in weight from one quarter of an ounce up to one ounce troy.

The colour was an extremely beautiful pure golden yellow, very much resembling that of the South American alluvial gold; and from its appearance it would be judged much richer in gold than it actually proved to be upon assay.

The specific gravity of the gold was determined upon 39·97 grains, carefully selected so as to be free from any visible mineral impurity, and was found to be 15·799 at the temperature of 60° Fahr.

The analysis was conducted in precisely the same manner as described in the first part of these researches when treating of the Welsh gold; but it was found, owing to the large amount of silver present, that considerable difficulty was experienced in getting the whole of the gold into solution; even when the gold

* In a letter from Mr. Wilson, dated the 8th of April, he informs me that he has already purchased gold from the diggers to the amount of £431.

had previously been flattened out extremely thin on an anvil, it became instantly coated with a firmly adherent film of chloride of silver, which effectually protected it from the further action of the acid; and it was only after repeatedly removing this film by dissolving it off with ammonia, that the entire amount of gold could be converted into the state of chloride.

Two analyses were made, and afforded respectively the following percentage results :—

Gold	81·11	81·27
Silver	18·45	18·47
Silica (quartz) . .	0·44	0·26
	100·00	100·00

From these numbers it will be seen that the composition differed very considerably from either the Clogau, Wicklow, or Cornish gold previously examined, and that it was alloyed with much more silver than even the stream-gold from the washings in the river Mawddach near Dolgelly, which by analysis afforded as much as 13·99 per cent. of silver.

The associated minerals, as far as I have been able as yet to verify, are :—quartz, which frequently is entangled in even the very small particles of gold; garnets, the iron alumina variety or almandine, of a pale red or rather pink-red colour, often crystallized in dodecahedrons and about the size of pins' heads; mica, iron glance, titanoferrite, magnetite, and iron pyrites.

Upon washing the gold from the lighter substances, there invariably remains a quantity of black sandy particles mixed with grains of quartz and garnets in fragments as well as in minute rounded dodecahedrons: upon treating this with a magnet the greater part of the black particles are removed; but there remain a number behind, which consist of black iron glance along with apparently non-magnetic titanoferrite. The black particles taken up by the magnet were examined and found to possess a specific gravity of 5·08 at 60° F., and to give a blackish-grey streak. On analysis they were proved to be composed of

Oxide of iron	91·26
Titanic acid	8·03
Silica . . . . . ,	0·71
	100·00

so that I imagine that the sand consists principally of grains of magnetite along with a little Ilmenite, or some other variety of titanoferrite.

Although the native gold from Sutherlandshire has as yet only been obtained from washing the alluvial deposits which have ac-

cumulated in the beds of the rivers, and not been mined out of the solid rock, the geological structure of the surrounding district furnishes evidence as to its origin. A glance at the maps published by Mr. Cunningham and Sir Roderick Murchison shows that the country is composed of slates, schists, and other highly metamorphosed rocks, now considered to be the representatives of the Lower Silurian formation, and further indicates that these strata are in many places broken through and disturbed by bosses and veins of eruptive granites.

Mr. Haswell informs me that his correspondents from Kildonan write him that the main rock is slate traversed by granite, and that china-clay is found along the banks of the burn, which appears to have been produced from the decomposition of the felspar in these granites; so that it may be inferred that the auriferous granite and the quartz and other dykes which invariably are connected with its intrusion have been the cause of the appearance of the gold itself, which most probably is of the same geological age as in Cornwall, Wicklow, and many other parts of both hemispheres.

Babingtonite.

This mineral species, which is essentially a silicate of iron and lime, is of extremely rare occurrence, perfectly authentic specimens having only been procured from its original locality (Arendal in Norway); and there it is but very rarely met with, as small crystals, associated with epidote and garnet in the iron mines. It is said to have been found in quartz in the Shetland Islands, and at Massachusetts on epidote; but in the latter case Dana, in the Supplement to the last edition of his ' Mineralogy,' p. 794, states that the angular measurements are not identical.

In 1854 I received from the late Mr. S. Blackwell a mineral which had been discovered in a railway-cutting in Devonshire in such quantities as to be used extensively as an iron ore, with the request that I should assay it for iron. The sample in question was found to contain 20·24 per cent. metallic iron along with 48·26 per cent. silica. The mineral, which looked much like a variety of hornblende, appeared to me to be worthy of further examination; but as I shortly after started for my foreign travels, I have been unable until lately to complete its examination.

The mineral itself occurs as an aggregate of groups of radiating crystalline fibres which curve in towards their centres of development, and possess a decided dark blackish-green colour; its lustre is vitreous, and in very thin splinters it is translucent; it is somewhat dichroic, and gives green and brown tints when examined by Haidinger's dichroiscope; fracture im-

perfect and hackly; brittle; hardness 5·5, scratching apatite, but scratched by orthoclase.

The specific gravity, determined by weighing a large specimen (2024·5 grs.) out of and in distilled water, was found to be 3·431. Another determination, made upon 38·26 grains in the powdered state, gave 3·436 spec. grav. at 60° F. After ignition for some time at a red heat, the mineral, which had lost 0·729 per cent., was found to possess a specific gravity of 3·474.

The blowpipe characters were found to be as follows:—Heated in a close or open tube it remains unaltered in appearance, but evolves a little water, which is probably only hygroscopic; if in larger fragments, it occasionally decrepitates. After heating, it reacts alkaline to reddened litmus and turmeric test-papers moistened with distilled water.

Heated alone in the blowpipe-flame, either in the platinum forceps or on charcoal, it melts at a pretty strong heat under 3 of Von Kobell's scale, quietly, without intumescence, to a brilliant black globule which is not magnetic; with soda alone in the reducing flame it also fuses with escape of gas to a black non-magnetic globule; on platinum-foil with soda and nitrate of potash in the oxidating flame it affords the characteristic green reaction of manganese.

With borate on platinum wire in the oxidating flame it dissolves completely to a clear reddish-yellow glass, which becomes colourless on cooling, and in the reducing flame on charcoal produces a greenish glass. With phosphate it gives the same colour-reactions as with borate, both in the oxidating and reducing flames; but in this case a skeleton of silica remains in the glass undissolved by the phosphate.

It is not, or at least very imperfectly, decomposed by acids. The chemical analysis was conducted as follows:—The amount of ignition loss was determined upon 38·40 grs., and after heating for twenty minutes to redness found to be 0·28 gr., equivalent to 0·729 per cent. 20·01 grs. of the mineral in fine powder were fused in a platinum crucible with 80 grs. carbonate of soda n potash; a strong manganese reaction was observed. The fused mass was decomposed by hydrochloric acid, evaporated in a water-bath to dryness, and then heated in an air-bath to render the silica insoluble; the silica, after being well washed and ignited, weighed 7·83 grs., or 49·12 per cent. of the mineral; its purity was tested by being dissolved, in a silver basin, in a solution of carbonate of soda, when only an inappreciable trace of oxide of manganese was found to remain insoluble.

The filtrate was thrown down by ammonia, and the alumina separated from the oxides of iron and manganese by potash; after reprecipitation and ignition it weighed 0·33 gr., equal to

1·604 per cent. The oxides of manganese and iron were then separated by carbonate of barytes and determined as usual, the sesquioxide of iron obtained amounting to 4·82 grs., and the manganosomanganic oxide to 0·27 gr., equivalent to 1·25 per cent. protoxide of manganese in the mineral. The solution, after separating the alumina and oxides of iron and manganese, was now precipitated by oxalate of ammonia, and the resulting oxalate of lime converted into carbonate by gently heating to redness with the subsequent employment of carbonate of ammonia. The amount of carbonate obtained was 7·46 grs., equal to 4·17 grs. lime, or 20·87 per cent. lime in the mineral. From the filtrate the magnesia was separated by addition of ammonia and then phosphate of ammonia, and the precipitate converted into pyrophosphate of magnesia by ignition ; it weighed 2·04 grs., equal to 0·73 magnesia, or 3·67 per cent.

As it was found that the iron contained in the mineral was present in the state of both protoxide and sesquioxide, it became necessary to determine their respective amounts. For this purpose a separate portion of the mineral was employed and treated as follows :—A portion of the mineral finely powdered was mixed with four times its weight of the carbonates of soda and potash, and placed at the bottom of a platinum bottle (beaten out of a single piece without soldering) about $3\frac{1}{2}$ inches deep and $1\frac{1}{4}$ inch in diameter ; a small porcelain tube like the stem of a tobacco-pipe was then suspended halfway down the interior of the platinum bottle, and through it a current of dry carbonic acid gas was kept passing, provided by a suitable apparatus ; the mixture of the mineral and carbonates was then heated until fully decomposed in this atmosphere of carbonic acid gas ; and after allowing it to cool, the fused mass was decomposed by the addition of sulphuric acid until all the iron was brought into complete solution, whilst during the whole of the operation the stream of carbonic acid gas was kept passing continuously through the apparatus, so as effectually to prevent oxidation. The contents of the bottle were now rapidly poured out into a beaker containing water which had been previously boiled to expel all air, and the bottle itself well rinsed with boiling water into the same solution, which was at once titrated by a normal solution of permanganate of potash in order to determine the amount of protoxide of iron present.

Two separate determinations were made in this manner, and the amount of sesquioxide of iron corresponding to the quantity of protoxide indicated was then subtracted from the total amount obtained in the course of the analysis, as before described.

The results of the analysis thus executed, when tabulated showed the composition of the mineral to be as follows :—

			Oxygen.	
Silica	49·12		26·19	
Alumina	1·60		0·75	3·68
Sesquioxide of iron . . .	9·78		2·93	
Protoxide of iron . . .	12·87		2·86	
Protoxide of manganese .	1·25		0·28	10·57
Lime	20·87		5·96	
Magnesia	3·67		1·47	
Loss on ignition	0·73		. .	
	99·89		40·44	

Upon calculation it will be found (when the smaller amounts of alumina, protoxide of manganese, and magnesia are taken into consideration, along with respectively the sesquioxide of iron, protoxide of iron, and lime) that we have 12 equivalents of silica, 1 of sesquioxide of iron, 3 of protoxide of iron, and 6 of lime; and taking the atomic numbers of these substances at $SiO^2 = 30$, $Fe^2O^3 = 80$, $FeO = 36$, and $CaO = 28$, we should have :—

$12\,SiO^2$	360	50·28
$1\,Fe^2O^3$ . . .	80	11·17
$3\,FeO$	108	15·08
$6\,CaO$	168	23·47
	716	100·00

These numbers, which approximate closely to those found by actual analysis as above stated, indicate the mineral to be composed of

$$6\,CaO\ SiO^2 + 3\,FeO\ SiO^2 + Fe^2O^3\ SiO^2,$$

or $\qquad 9(CaFe)O,\ SiO^2 + Fe^2O^3\ SiO^2,$

giving the general formula

$$9\,RO\ SiO^2 + R^2O^3,\ 3\,SiO^2,$$
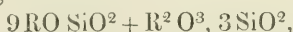
in which the protoxides are those of iron, manganese, lime, and magnesia, whilst the sesquioxides are alumina and sesquioxide of iron.

In the analysis of the Babingtonite from Arendal, spec. grav. 3·366, Rammelsberg[*] obtained the following percentage results :—

		Oxygen.	
Silica	51·22	26·59	
Sesquioxide of iron . .	11·00	3·30	
Protoxide of iron . . .	10·26	2·28	13·19
Protoxide of manganese .	7·91	1·78	
Lime	19·32	5·52	9·89
Magnesia	0·77	0·31	
Ignition loss	0·44		
	100·92		

[*] *Handbuch der Mineralchemie*, p. 477.

from which he also deduced the same formula,

$$9 \, RO \, SiO^2 + Fe^2 O^3, \, 3 \, SiO^2,$$

being

$$9 \left. \begin{array}{l} 1\frac{0}{17} \, CaO \\ \frac{4}{17} \, FeO \\ \frac{3}{17} \, MnO \end{array} \right\} SiO^2 + Fe^2 \, O^3 \, SiO^2.$$
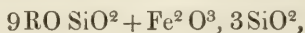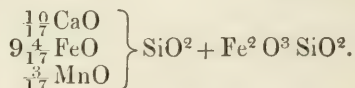
The minerals are also perfectly identical in constitution, the only difference being that the percentage of manganese in the English Babingtonite is considerably less than in the specimen from Arendal analyzed by Rammelsberg, which also contained no alumina. The Arendal Babingtonite, however, appears to vary considerably in composition in different specimens; for its analysis by Arppe* afforded him only 1·8 per cent. protoxide of manganese along with 0·3 per cent. alumina, whilst another analysis by Thomson† showed as much as 6·48 per cent. alumina and 10·16 per cent. protoxide of manganese. As, however, neither of these chemists had determined the state of oxidation of the iron in the mineral, the numerical results of their analyses are useless for the purpose of determining the constitution or formula of the mineral itself.

Children announced that titanic acid was a constituent in this mineral, which I could not confirm; and Arppe's explanation, that some of the Arendal crystals contained minute particles of titaniferous magnetite, no doubt accounts for this statement, since, after extracting all magnetic particles from the powdered mineral before analyzing it, Arppe found that no titanic acid could then be detected in the purified silicate.

XLV. *Lecture Experiments to illustrate the Laws of Motion.* By Professor Robert Ball, *A.M.*‡

I HAVE found the arrangements described in this paper effective in demonstrating to an audience a few elementary properties of gravitation and the laws of motion. It is certainly true that a clear appreciation of the truth of these laws, so essential for properly studying dynamics, requires some experimental illustration to beginners. However satisfactory may be the multitudes of indirect proofs of these laws with which the more advanced student is familiar, it will hardly be denied that it is a little difficult to demonstrate them directly. So far are they from being axiomatic, that for centuries they were not believed at all.

* Berzelius, *Jahrbuch*, vol. xxii. (1842).
† Phil. Mag. vol. xxvii. p. 123.
‡ Communicated by the Author.

The apparatus which is described in works on physics as available for the purpose of proving them, consists merely of a very few instruments, among which Atwood's machine is that most commonly referred to. This meagreness contrasts strangely with the profusion of apparatus which has been devised for the illustration of the elements of other branches of science (electricity, for example); yet surely it would be as desirable to prove to a student the second law of motion by direct experiment, as it is to demonstrate experimentally the laws of electrical induction. I have communicated this paper, thinking that any contribution, however small, to the list of apparatus available for this purpose might prove useful to others, as it has been to myself.

A certain principle should always be borne in mind by the arranger of an experiment which is to be used for purposes of elementary instruction. A law A is required to be proved; and an experiment is performed which demonstrates the fact B; it is then shown by logical inference that the truth of the fact B necessitates the truth of the fact A. Thus A has been proved by two distinct steps—(1) the experiment proving B, (2) the chain of reasoning connecting A with B. For the instruction of a class of beginners, it is essential that the experiment be so selected that the second step be reduced to a minimum, or, if possible, totally dispensed with. To take an illustration from the subject with which we are engaged. Suppose it be required to prove that a body will fall sixteen feet in the first second. The distance may, of course, be determined most accurately by finding the time of vibration of a pendulum; but the subsequent chain of reasoning would be quite unintelligible. Atwood's machine might be made to give some approximation to the value; but it likewise does not indicate the direct result, but rather something from which the direct result is to be inferred by calculation. This appears to me one of the defects in Atwood's very beautiful machine: it possesses neither great accuracy nor great simplicity in the interpretation of its results; and one or the other, if not both, of these features should characterize every experiment. Besides, in its ordinary form Atwood's machine is quite unadapted for use in the lecture-theatre.

The arrangements now to be described have been designed with a view of proving the points required as directly as possible and with the minimum amount of subsequent reasoning.

It should be mentioned that whatever has been necessary for the purpose of supports and framework has been constructed out of the very beautiful system devised by Professor Willis, and described in his 'System of Apparatus for the use of Lecturers and Experimenters in Mechanical Philosophy' (Weale and Co.).

The convenience of this apparatus for every kind of mechanical appliance in a lecture-room is wonderful.

All bodies fall through the same height in the same time. This was Galileo's experiment from the top of the tower of Pisa ; it can be repeated on a small scale in the lecture-room in a striking manner.

A scaffolding is built up with the stool, beds, and bolts of the system ; and thus at the height of 24 feet from the floor a pulley is supported through which passes a rope. A piece of wood the shape of an equilateral triangle, 9 inches each side, has two electromagnets, each 3 inches long, attached to its base, the wire being continuous round the two magnets. The triangle is attached by its vertex to the rope, and can therefore be easily raised to 24 feet, or any less height, and lowered again at pleasure. The two wires communicating with the electromagnets are sufficiently long to allow of the triangle and magnets being hoisted up while the other ends of the wires are attached to a battery of a few cells, a contact-breaker being introduced into the circuit.

The mode of experimenting will be easily understood. Suppose it is desired to prove that a heavy iron ball and a light one will fall through the same height in the same time. The circuit being complete, the balls are attached one to each of the electromagnets, care having been taken to interpose a piece of paper between each of the balls and the corresponding magnet, as this ensures their being disengaged simultaneously. The piece of wood and its freight are then hoisted up 24 feet (or less), and the two balls are in the same horizontal line, supported, of course, merely by the attraction of the magnets. At a signal the current is broken and the balls fall together ; they are disengaged simultaneously ; and the line joining them is easily seen to be horizontal throughout their entire descent, though, of course, they are perfectly free from each other. The iron balls used were 1 inch and $1\frac{1}{2}$ inch in diameter respectively.

An iron ball ($1\frac{1}{2}$ inch) and a cork ball ($2\frac{1}{2}$ inches) can be likewise tried. A flat-headed nail driven into the cork affords sufficient holding-ground for the magnet. It is a most unexpected result to find that when they reach the ground, or rather the cushion placed to receive them, the cork ball is only a few inches behind its weighty companion. That even this difference is due to the resistance of the air is shown in the next experiment, by lowering down the triangle, again affixing the cork ball, and likewise the iron ball, with a small parachute of cardboard attached to it. Raising up the triangle and again breaking the current, the cork is seen this time to reach the ground before the iron.

The next experiment is to prove that a body falls sixteen feet in the first second. The apparatus already described is employed for this purpose, but into the same circuit two other parts are introduced which will require a few words of explanation.

The contrivance on which the arrangement principally depends is the contact-breaker, the action of which will be understood from the annexed diagram (fig. 1). To a block of wood, A B, a

Fig. 1.
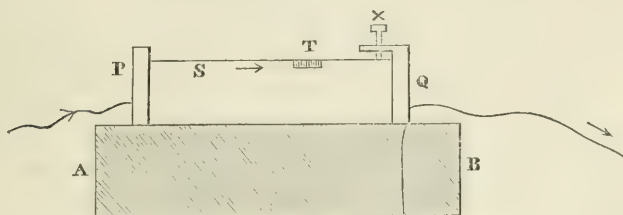
brass pillar P is screwed. This pillar is 3 inches high, and has a binding-screw attached to it to receive the current. Near the top of the pillar a very slender spring is rivetted; this spring is of brass wire slightly flattened, and is 8 inches long; at the point T, five inches from P, it bears what may be called a saddle. This consists of a piece of ordinary tin plate cut into a rectangle of 1 inch by $\frac{1}{2}$ an inch, soldered lengthways on the upper surface of the spring and then bent down on each side, so that its section is similar to what is represented in fig. 2. The object of this will presently appear. The other end of the spring is free, but it bears against a screw, X, which turns in a brass piece, Q, likewise screwed to the block of wood A B. The spring being

Fig. 2.

weak enough, the slightest touch will depress the end of it from X, to which, however, it immediately returns on the relaxation of the pressure. When the spring (along which the current travels) touches X the circuit is complete, and it is of course interrupted when the spring is depressed.

A seconds-pendulum is suspended from a suitable portion of the framework by a spring in the usual manner. No clock-escapement is used; indeed, if the bob be heavy, the pendulum once set in motion will vibrate for some minutes without requiring an additional impulse. Underneath the pendulum the contact-breaker is to be placed in such a manner that its spring is normal to the plane of vibration of the pendulum; and the height of the pendulum must be so adjusted that when the bob is in its lowest position, a point attached to it shall just touch the saddle in passing over it. The final adjustment, however, is inconvenient to make by moving the point of suspension of the heavy

pendulum ; so, for the sake of making this with the necessary
delicacy, the screw X has been introduced into the contact-
breaker, by raising or lowering which the limiting position of
the spring and therefore of the saddle is raised or lowered.
By this means the amount by which the pendulum depresses
the spring in its transit over it can be arranged with the great-
est nicety. After the pendulum has received an impulse, and at
the bottom of its swing comes into contact with the saddle, the
current is broken owing to the depression of the spring from X.
After the point has passed the saddle the spring returns to its
bearing, and the current flows again until the return of the
pendulum to the lowest point, when the current is broken again ;
and if the contact-breaker have been nicely adjusted, exactly a
second will have elapsed between these two breakings. Thus at
the completion of each second the current is interrupted. The
apparatus being nicely adjusted, the amount of *vis viva* lost by
the pendulum in depressing the spring is so small that it will
make, after having received an impulse, upwards of two hun-
dred breakings before it requires another push.

An electromagnet acting on a bell is introduced into the cir-
cuit, so arranged that at each interruption of the current the bell
is struck. The simple arrangement necessary for this need not
be described. At every oscillation of the pendulum the bell
rings, the sounds of which may therefore be regarded as the ties
of the pendulum rendered easily audible to the whole room.

The reason why the spring or middle of the saddle of the con-
tact-breaker should be placed exactly under the point of suspen-
sion of the pendulum can now be easily seen. It is in order
that the intervals of two consecutive strokes of the bell shall be
exactly equal. If the spring have not the correct position, then
there will be two intervals, one as much greater as the other is
less than a second, and these intervals will alternate. Errors
which arise from the want of perfect adjustment of this position
are fortunately rendered insensible by the fact that at its lowest
point the pendulum has its maximum velocity.

It will be noticed that with this contact-breaker the interval
between the strokes is independent of the arc of vibration.

I am not aware that this mode of breaking contact has been
used before, and therefore I have given the details. It ap-
pears convenient, efficient, accurate, and not liable to derange-
ment, and is free from the troubles (in my experience unavoidable)
arising from using mercury or other fluids for the same purpose.

The mode of proving that a body falls sixteen feet in a second
is then easily seen. A large scale divided into feet is attached
to the upright support of the pulley. One or both of the elec-
tromagnets belonging to the triangle, the contact-breaker, and

the bell are all included in a battery of a few cells. The pendulum being held to one side (for in its position of rest the point keeping down the saddle breaks the current), the current passes. An iron ball is attached to the electromagnet, which is then hoisted to the height of sixteen feet (as pointed out by the scale) above the surface of a cushion on which the ball is to fall, so as to deaden the sound. The eyes of the audience are to be directed to the cushion, while their ears listen for the bell. When the pendulum is released, the first break rings the bell and drops the ball together; the second break rings the bell again, and, as nearly as the eye and ear can judge of simultaneity, identically at the same moment as the ball reaches the cushion. If the ball be hoisted to the height of eighteen feet, it is seen to be too late; if lowered to fourteen feet, it is seen to be too soon. From this may be inferred the amount of accuracy of which the experiment is capable. It must be remembered that the arrangement is one for illustrating a certain quantitative result to an audience, and not for determining in the cabinet an important natural constant.

The next experiment may perhaps claim greater accuracy than that just described, as there is always more or less of difficulty (more, apparently, with some persons than others) in perceiving the identity of time of two phenomena presented to two different senses simultaneously. This arrangement depends only on the sense of sight; and the law which it demonstrates may be thus enunciated.

"A body projected in a horizontal direction with any velocity whatever, will take the same time to reach the ground as a body let fall vertically through the same height."

This clearly is an important truth to impress on a beginner endeavouring to understand the second law of motion. It will teach him that, in at all events one very important case, the effect of the same force acting for the same time does not in the least depend upon the circumstance as to whether the body on which it acts is at rest or in motion; and perhaps this is the least self-evident of the truths that are wrapped up in Newton's concise enunciation of his law.

The arrangement consists of two parts, and the assistance of electricity as a rapid messenger is again called in. A B C D (fig. 3) is a piece of wood $1\frac{1}{2}$ inch thick; A C is an arc of a circle of two feet radius, the tangent to which at C is horizontal, A C being about a foot long. A ball being intended to run down this piece, it is grooved; and A C is one of the edges of the groove, the other being of course at a distance of $1\frac{1}{2}$ inch on the other side of the wood. The ball which rolls down the groove is a sphere of $2\frac{1}{2}$ inches in diameter, made of wood and neatly covered over with tinfoil. The two edges of the groove are each

likewise covered with a piece of tinfoil, which pieces, however, must at no place communicate with one another.   Each edge is

Fig. 3.
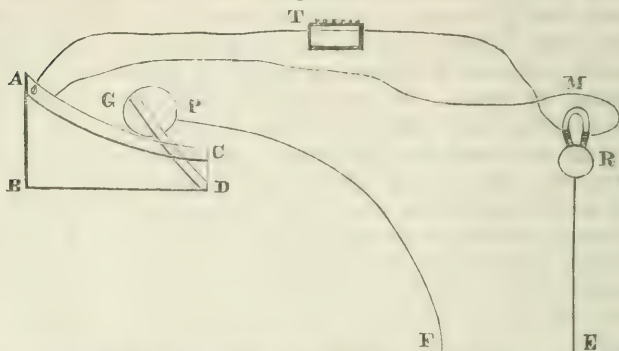
furnished with a binding-screw, to which a wire is attached. Whenever the ball rests on the groove, the tinfoil enclosing it touching each edge completes the electric connexion between the two binding-screws, the ball acting as a bridge along which the current passes.   At D, and the similar point on the other side, the two ends of a piece of india-rubber spring are fastened so that the ball can be grasped by the spring.   When the ball is pulled up along the groove and then released, the force of the spring pulls it down and it darts off with a horizontal velocity. This piece of apparatus may be about seven feet from the ground. At precisely the same vertical height as C, and at a distance of some feet, an electromagnet, M, is to be supported.   One wire from this goes to the battery, the other is fastened to one of the binding-screws on one edge of A C, the second edge being connected with the other pole of the battery.

As long as the tinned ball P is on the groove, the circuit being complete, the electromagnet, M, will sustain a second ball R ; but the moment P leaves the groove, R is released.   Drawing back P and the spring which embraces it and then releasing it, P maintains the circuit until it arrives at C ; in fact a good contact is ensured by the double circumstance that both the spring and the centrifugal force of P conspire to keep it in close contact with the tinned edges.   After leaving C the ball darts off in the trajectory indicated in the figure ; but directly it is free R is likewise free, and the two can be seen with the greatest exactness to reach the ground together.

By stretching the spring more or less, any amount (within reasonable limits) of horizontal velocity can be communicated to P ; and it is a most striking result to observe in all cases the perfect simultaneity with which the two balls reach the ground.

For demonstrating the particular point in question, this arrangement apparently leaves little to be desired.

"The path of a projectile is a parabola." The method of demonstrating this is indicated in M. Daguin's *Traité de Physique* (vol. i. p. 94). The arrangement for the lecture-room based upon it is simple enough. A quadrant (fig. 4) of two feet radius is made of wood $1\frac{1}{2}$ inch thick, and grooved as in the preceding experiment. This is to be very firmly supported parallel to the wall and about two inches distant from it, and six or seven feet from the ground. Down this a wooden ball $2\frac{1}{2}$ inches is to roll, and it is proposed to prove that the path it follows after leaving the groove is a parabola. That the ball after rolling down shall describe precisely the same path each time, it

Fig. 4.
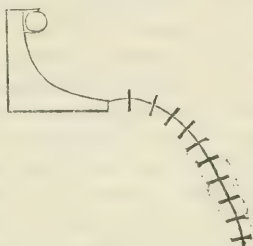
is necessary that the direction of projection be perfectly constant; this is ensured by fixing the quadrant very firmly; and then the direction of projection is the tangent at the base, which for convenience is horizontal, or nearly so. It is no less necessary that the velocity of projection be constant; this is provided for by allowing the ball always to start from the same position on the quadrant. To secure this, a small ledge is fastened at the top, and the ball is, before each descent, brought home against the ledge, and thence allowed to fall. By this means the same trajectory can be reproduced as often as desired.

To show that this is a parabola, the following simple plan is employed. A number of little arches are made from slips of cardboard 1 inch wide; these arches are about 4 inches across and 6 inches high, something the shape of the letter U turned upside down. They are fastened to the wall by drawing-pins or otherwise all along the constant path traversed by the ball, as shown in the figure. The mode of placing them is easy. First, one is arranged so that after each descent the ball goes through its centre; then the next is similarly placed, and so on until ten or thereabouts have been affixed, through all of which, and without touching any, the ball will pass, after leaving the curve, finally falling into a basket placed to catch it.

By joining the centres of the arches along the wall by a curve, the position of the focus and directrix of the parabola will be easily found, and the nature of the trajectory consequently demonstrated.

Royal College of Science for Ireland,
  Stephen's Green, Dublin,
    March 25, 1869.

XLVI. *On Shadow Optometers.* By John C. Douglas, *East-India Government Telegraph Department**.

THE well-known experiment of Father Sheiner, in which a small object is regarded through two small orifices, is the principle upon which almost all optometers are constructed. The distance obtained is generally considered the mean distance of distinct vision. Repeated experiments do not give similar results with the same person; and the use of the principle is stated to be only possibly of value when great practice has given the faculty of altering accommodation†. The variation between the results of several determinations is not such as to destroy the value of these results; a variation might be expected. The objection most fatal to its employment appears to me to be the indefinite *nature* of these results. In using Sheiner's method to examine the nature and position of images on the retina, and to find the conjugate foci under different conditions of accommodation and refraction, the interposition of the diaphragm is a condition which removes the circumstances of the case from the conditions of natural vision. I venture to propose the following as far preferable to Sheiner's experiment for optometric purposes.

If a small object be moved to and fro between the eye and two or more luminous points not having conjugate foci on the retina, and subtending a small angle with each other at the eye, as many objects or rather shadows will be seen as luminous points employed, except at a particular distance, at which the object appears single: pin-holes in a card held before a lamp or distant street-lamps may be used to furnish the luminous points, and a common pin or pen-nib may be employed as the object. The cause of this multiplication of the object is evident; the objects seen are shadows of the object employed, one shadow being given by each light. As there is only one object, if this were placed in a focus of the eye having its conjugate on the retina, it would appear single and distinct. Of the infinite number of parallel planes of the pencils between the eye and the sources of light only one is at a focus having its conjugate on the retina in any given state of accommodation. If the object be in this plane, as the section of the pencils through this plane is accurately reproduced on the retina, the images of the several shadows will coincide and one only be seen; but if the object be placed on any other plane the several shadows will no longer coincide on the retina, and a separate shadow due to each light will be seen. A method is thus furnished for finding for what distance the eye is accommodated without the disturbance due to the interposition of a diaphragm between the object and the eye.

If A be a convex lens, B a focus of diverging rays, C an opaque object interposed between B and A, and 1, 2, and 3 a screen
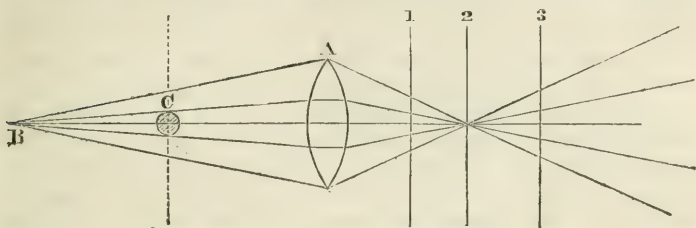
receiving images at the different positions shown, it will be apparent on inspection of the diagram that if the object C be moved in the direction of the dotted line, the shadow will on 1 move in the same direction as the object, and on 3 in an opposite direction, while on 2 no shadow will be thrown, but the luminous point B will be reproduced diminished in intensity. If the lens be replaced by the refracting media of the eye*, and 1, 2, and 3 be positions of the retina, it will be evident that with the retina too near A for distinct vision of B, (1) the motion of the shadow will be seen in the opposite direction to that of C; and with the retina too distant (3), it will appear to be in the same direction : with the retina at 1 the shadow of C will appear inverted with respect to C; with the retina at 3 it will appear erect. The different relative positions of the retina may be due to any cause altering either the refracting media or the figure of the eye, or both, the diagram being a convenient mode of representation only : *e. g.* transfer of the retina to 1 may represent a diminution of the refractive power of A, the retina not moving, &c.

Applying this to optometric purposes, a method is afforded of determining the effect of accommodation and refraction at any moment with reference to any luminous point B. To find if the retina is too near or too distant from the lens for distinct vision, pass a pin or pen-point past the eye close to the cornea; if no shadow is seen, the eye is accurately adjusted; if a shadow is seen and moves in the same direction as the object, the retina is too far from the lens; if the shadow be inverted and move in the opposite direction, too near; if the luminous point be far enough distant for the rays to be considered parallel, the three cases are emmetropia, myopia, and hypermetropia respectively.

The degree of myopia or hypermetropia may be measured by interposing lenses until the shadow is no longer seen, or lenses may be applied and the luminous point approached or withdrawn until the shadow is no longer seen; myopia may be measured by merely approaching or withdrawing the luminous point with-

* A diagrammatic eye for simplicity.

out the interposition of a lens; but hypermetropia cannot be so measured, there being no positive distance at which vision is perfect. If a graduated series of lenses be not at hand, it must be first overcorrected by a lens, and then the distance of the focus of incident rays adjusted until the shadow is no longer seen.

If two or more luminous points be used, they are removed until the shadows are erect and move in the same direction as the object (a convex lens being employed in hypermetropia). The object being held near the eye, as many shadows are seen as lights employed; the object is now removed until one shadow only is seen; the distance at which this occurs, and the effect of any lens introduced being known, the distance required may be at once arrived at.

On comparing the methods last referred to with Father Sheiner's method, I found in my own case a difference of one inch in favour of the proposed method in determining the far point—Sheiner's method giving rather more than seven, and the multiple-shadow method proposed more than eight inches. I found, in Sheiner's experiment, that practice enabled me to render the object confused after it had apparently reached the far point, and that it became again distinct after further removal; but this distance was that referred to above, and was not the far point; for on regarding an approaching point of light, I found the distance thus obtained agreed with the shadow indication, and exceeded therefore the distance given by Sheiner's experiment. In some hundreds of experiments Sheiner's method invariably gave a shorter distance; the interposition of a concave lens removed the far point obtained by the shadow method to twelve inches; but that obtained by Sheiner's method under the same circumstances was about an inch less than this distance.

The method proposed appears to have the following advantages over Father Sheiner's experiment: it appears more accurate; vision is natural, not being modified by the introduction of a diaphragm; it is at once seen what kind of correction is required; and the far point may be more readily found. Most persons accommodate for their far point only when actually viewing a distant object; and the conditions of Sheiner's experiment are therefore unfavourable to the determination of the far point; but no unfavourable conditions are introduced in the methods proposed, a distant object being viewed. It also appears superior to the use of test-types, being more accurate, showing more certainly (to unscientific persons at least) the existence of confusion and the kind of correction required. In the last two respects it is superior to the method recommended by Donders, viz. viewing a point of light through lenses, the passing of the object showing at once if perfect adjustment has been arrived at, and,

if not, the kind of correction required, both of which, without the application of the shadow method, must be left to trial and the opinion of the observer, which (particularly in the case of his being inexperienced) must render the process tedious and the result more or less inaccurate.

As the methods described depend on the passing of a shadow over the field, and are thereby distinguished from other methods of determination in which the object is regarded directly, I have called them shadow methods; and I suggest the name Shadow Optometer as appropriate to such instruments constructed on these principles. The purposes to which the principles described and explained are applicable, and the manner of applying them, are evident; the special arrangements in any particular case (upon which the accuracy of any method must in a measure depend) will depend on the ingenuity of the experimenter. Possession of a pen-nib and a set of lenses would enable any ordinarily intelligent person to find the kind of spectacles he required (if any) with great exactitude by viewing a distant street-lamp in the manner indicated.

After completing the above, I found the inversion of a small object placed near a hole in a card held near the eye had been described by M. le Cat in his *Traité de Sens* (Brewster's Edinburgh Journal of Science, vol. iv. p. 89); but his description is incomplete as given in Brewster's Journal of Science, and his explanation appears to me erroneous. It is stated the pin and hole must be near the eye (ibid.), whereas any luminous point may replace the hole in the card, and the phenomenon depends on the distance of the card being less than the focus of the eye having its conjugate on the retina at the time of experiment. The explanation given is, that the shadow of the pin is seen, and the inversion is due to the fact that the light from the lower part of the white wall or window, which furnishes the light in M. le Cat's experiment, passes by the upper part of the pin, and that from the upper part passes by the lower part of the pin; hence the shadow is inverted with reference to the wall or window. I consider the correct explanation is that given by me, and that inversion of the light from the wall or window with regard to the pin has nothing to do with inversion of the shadow, which is produced equally well if a luminous body replace the hole in the card. I have not noticed that the shadow appeared to be on the opposite side of the card to the eye; and the diagram given in the notice referred to (ibid.) is evidently incorrect; it removes any possibility of the explanation given being that given by me. The enlargement of the shadow is due to the same cause as would enlarge any shadow modified by the effects of refraction, and not to any circumstances peculiar to this case, as shown in the diagram referred to.

Calcutta, Feb. 7, 1868.

XLVII. *On the Cause of a Pink Colour in White-Lead Corrosions.* *By* William Baker, *F.C.S., Associate of the Royal School of Mines, London*.*

IN some contributions to the metallurgy of lead, published in the Philosophical Magazine in 1862, I attributed a certain pink tint, occasionally seen in white-lead corrosions, to the presence of small quantities of copper. As the results of any experiments upon the corrosion of lead by the combined action of fermenting bark, acetic-acid vapour, and atmospheric oxygen can only be arrived at after the expiration of ten weeks or three months, the progress of further investigation in this matter has been necessarily slow. I have been unable to isolate the colouring-matter; but I wish to correct the statement that the pink colour is due to copper, and to detail some conclusive proofs that it is caused by finely divided silver.

Having obtained many tons of lead which contained only traces of copper, I found in several instances the pink colour still quite evident in the corrosions. By the method which had been employed for refining the metal, there could be only silver left as an impurity in any perceptible amount. I therefore sought for evidence that this substance could produce such a result. Upon analyzing 5000 grains of a perfectly white corrosion, and one which was distinctly and uniformly pink, the result showed that the composition of the two samples differed mainly in the amount of silver:—

	CuO.	FeO.	NiO.	Ag.
White corrosion	·0050	·0022	trace	·0005 per cent.
Pink corrosion	·0060	·0022	·0013	·0058 ,,

A small quantity of silver was then added to a portion of the lead which had produced the white corrosions, and this was again submitted to the corroding action. The result was a decided pink carbonate. This synthetical experiment was repeated many times with a like result upon various samples of lead which had before produced a white carbonate; and I find that the pink colour begins to show at the edge of the metallic portion left uncorroded when the silver amounts to more than $\frac{1}{2}$ oz. per ton of lead. A decided colour, which is uniform throughout the mass of the corrosion, is obtained when the silver amounts to about $1\frac{1}{2}$ oz. per ton. A fracture of a dense corrosion often shows the crystalline character of the metallic lead, which is defined to some extent by the pink colour—as if the silver had

* Communicated by the Author.

segregated out at certain faces of the lead crystals. By the addition of a small quantity of arsenic or antimony the pink colour was replaced by a dull purple; and a clear pink tint was only obtained when all the oxidizable metals had been removed.

I come now to the discussion of the state in which the silver exists to cause a pink or reddish reflection of light. Silver does not oxidize under the conditions of exposure to acetic-acid vapour and oxygen of the air. Moreover oxide of silver and silver carbonate are themselves decomposed and reduced to a metallic state by a heat below that attained in the stacks of fermenting tan. The silver must consequently be in the metallic state. As confirming this statement I made the following experiments :— Silver carbonate was triturated with white lead and water and then dried. Upon increasing the temperature, a delicate pink tint became visible upon the reduction of the oxide of silver. If a small quantity of silver carbonate be precipitated along with lead carbonate, the colour upon drying and heating is more uniform, and it may be obtained exactly resembling the tint seen on white-lead corrosions.

The colour of the photographs obtained by means of silver-salts is also evidence in favour of the metallic state of the silver; and I may also adduce the fact that a ray of light, when reflected ten times from a polished silver surface, is distinctly of a reddish colour.

Collegiate Laboratory, Sheffield,
April 19, 1869.

XLVIII. *On the Colour of the Lake of Geneva.* By L. Soret*.

My dear Tyndall,   Geneva, March 31, 1869.

IN thanking you for your letter and the pamphlet you sent me, I take the opportunity of communicating to you an observation which may interest you.

Whilst dealing with a different subject, I was led to consider whether the blue colour and the absorption of certain rays of light by water are due to the liquid itself or to the therein suspended solid particles. This question has been often discussed by others, as well as by yourself in your work 'The Glaciers of the Alps.' Your memoirs on the polarization of the blue light of the sky have suggested to me the idea that if the blue colour of the water be due to suspended solid particles, phenomena of polarization will be produced analogous to those observed by you on the light of the sky.

The water of the Lake of Geneva, owing to the well-known

* Communicated by Professor Tyndall, F.R.S.

beauty of its colour, is very favourable to investigations of this kind.

For this purpose I had a very simple apparatus constructed. It is a kind of telescope; a flat plate of glass with parallel surfaces, fitted hermetically at its one end, serves as *object-glass*. The instrument, therefore, can be immersed in water without the latter being able to enter it. The *eyepiece* consists of a Nicol's prism. It is easy to understand that, by immersing the telescope in the water, the eye receives the blue rays of light emanating from the water, and that this light can be analyzed by turning the Nicol.

By proceeding in this manner, I found that the water of our lake really exhibits phenomena of polarization comparable to those observed on the light of the sky; only their observation is more difficult, and up to this time I have not been able to study them as well as I could have wished.

Supposing the surface of the water N N perfectly plane, which
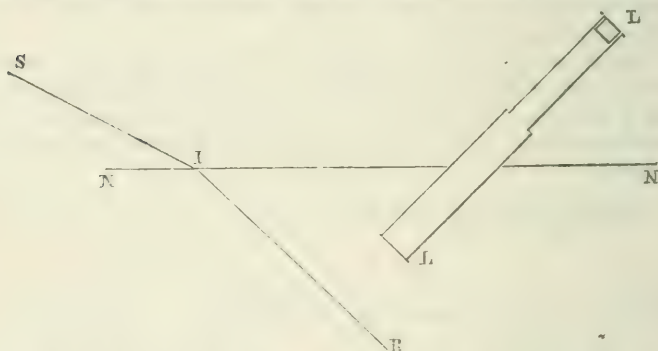
is the case in time of perfect calm, a beam of solar light incident with the direction S I will be deviated to I R after the refraction. Now from a boat the telescope can always be placed in the vertical plane passing through the sun. If the telescope be inclined in such a manner that its axis becomes perpendicular to I R, then the light received by the eye is emitted perpendicularly to the direction of the solar rays in water. This arrangement is analogous to that where the maximum of polarization of the light of the sky appears—that is, when one is looking at a right angle from the sun.

In this manner I made a series of observations on the Lake of Geneva in a place where the water was sufficiently deep not to allow the ground to be seen; and I was able to perceive a marked polarization. The plane of polarization was coincident with the plane of incidence.

If the telescope, always in the same vertical plane passing through the sun, be inclined in any other direction, then the polarization is the less marked the more the direction of the telescope differs from the perpendicular to I R. If the direction of the telescope be vertical, so that one is looking downwards, no trace of polarization appears.

If the telescope be placed in a vertical plane perpendicular to the former one (that is, at 90° to the sun), the polarization is the more distinct the more the direction of the telescope becomes horizontal, and the plane of polarization passes through the direction of the telescope and the sun.

I have not been able as yet to prove that the maximum of polarization corresponds exactly to the position of the telescope where its axis is at a right angle to the direction of the solar rays.

It cannot escape your attention that the phenomena are more complicated in the case of water than in the case of the firmament.

In the first place, it is evident that, if the surface of the water is agitated, the solar rays cannot remain parallel after the refraction. The phenomenon, therefore, is the less distinct the more agitated the water is. This is exactly what I observed. When I tried my apparatus for the first time, the water was agitated, and I remarked no appreciable polarization—although this may be attributed as well to the fact that, for some days before, we had strong northerly winds and the water was not perfectly transparent. On two other days, when the water was only slightly agitated, an appreciable polarization could be observed. On a very fine and calm day at last, the polarization was as marked as that of the sky, which, however, was not very blue at the time.

In the second place, the solar rays entering the water are already partially polarized by refraction; but when the telescope is placed in the vertical plane passing through the sun (that is, in the position most favourable for observation), it is easy to see that the rays already polarized must be extinguished (exactly as in the experiment where you produce a *blue cloud* with a pencil of light already polarized).

Finally, the direct solar light is not the only light that enters the water. There are scattered rays of diffused light, which, impinging upon the water from all directions, produce after refraction a blue non-polarized light, or, properly speaking, an infinite number of rays polarized with different planes of polarization. I have satisfied myself that, when the sky is covered, no appreciable polarization is observed.

As I know of no prior description of this phenomenon, and as peculiar circumstances prevent for some time my pursuing this investigation, I communicate to you these results, although I

regret that I was not able to finish the series of observations, and to repeat them with the use of artificial light.

<div align="right">Yours &c.,

L. SORET.</div>

I hope my friend Soret will compare the action of the water of the Lake of Geneva upon light with that of other waters. By intensifying his illuminating beam he may be able to operate upon small masses. His method of experiment holds out a promise of a definite solution of a much discussed and still open question.

An elaborate memoir, " Sur la Polarisation Atmosphérique," published in 1864 by Dr. Rubenson of Upsala, has just reached me. I promise myself much instruction from the perusal of this essay.

Royal Institution,   J. TYNDALL.
April 21, 1869.

XLIX. *Fundamental Principles of Molecular Physics.* *By* Professor J. BAYMA, *S. J., of Stonyhurst College.*

[Continued from p. 287.]

III.

IN his paper on the "Fundamental Principles of Molecular Physics" Professor Norton undertakes not only to answer my objections against his theory, but also to show, as far as he can, that some of my own views on the same subject are questionable, and others inadmissible. Having in my last article examined briefly his system of defence, I come now to a rapid review of his means of attack.

The reader, if he has watched attentively the progress of our controversy, will have already noticed the striking ability displayed by my learned opponent in framing arguments out of objections. The last example of such tactics I have reserved for this part of my reply as a natural introduction to what I shall say concerning his other arguments.

Bulk of atoms.—As Professor Norton had assumed the atom of "gross matter" to be indivisible and spherical in form, I took the liberty to object that atoms " indivisible " cannot be either extended or spherical in form ; for " if they were extended and indivisible, they would be so many pieces of continuous matter, which we have already proved to be impossible."

To this the learned Professor gives no less than six distinct replies, which I am now going to examine. The first is as follows :

" Professor Bayma *assumes* that every point of matter acts instantaneously upon every other point at all distances, however great or small, with a force having the same character at all distances, and inversely proportional to the square of the distance.   This may be probable, but *it is not self-evident* ; and in fact *no reason can be assigned* why one material point having no extent should act upon another with a force decreasing with the distance according to any law whatever.   The law of inverse squares is a consequence of *wavepropagation*, or of radiation along definite lines, received *on a molecule of definite size*, and *cannot* be predicated of a force that acts instantaneously between two *mathematical* points.   To suppose such a law is *an arbitrary assumption*."

I beg leave to make some remarks upon the few expressions which I have italicized.   1st. The word *assumes* should be changed into *proves*. (See Molecular Mechanics, pp. 31, 32, and 53–65.)   2nd. *It is not self-evident* : of course ; and therefore it was made evident by the help of special proofs.   3rd. *No reason can be assigned* : and yet many were assigned, and others are still assignable.   4th. *Wave-propagation* is a propagation of motion, and has nothing to do with elementary action, which cannot be propagated (Molecular Mechanics, pp. 63–65).   5th. *On a molecule of definite size*.   Continuous or not ?   If continuous, then the reply confirms my objection : if not, then the action is received on single material points, contrary to the assertion of my learned critic.   6th. *Cannot*.   Why not ?   7th. *Mathematical points* : mathematical does not here exclude *physical*.   8th. *An arbitrary assumption* : here the learned Professor gives himself the innocent pleasure of applying to me, by way of retaliation, what I ventured to say and to prove of some of his fundamental principles.   Fortunately however those who have read my ' Molecular Mechanics ' know that I have done enough not to deserve the compliment.   I wish my learned opponent had done as much.

But, even setting aside all these imperfections, the reader will undoubtedly see that this first answer of the learned Professor is not calculated to meet my objection.   Accordingly I consider all further discussion of it as unnecessary.

His second answer is the following :

" If matter consists of material points, as supposed by Professor Bayma, it is no more difficult to conceive of an atom of continuous matter than of the space coextensive with it."

This second answer I cannot well understand.   Surely, the learned Professor does not mean that, *if* matter (as I have not only *supposed*, but *proved*) consists of separate material points, *then* continuous matter can be more easily conceived.   Yet what else is the natural sense of his conditional proposition ?   How-

350 Prof. J. Bayma *on the Fundamental*

ever this may be, he wishes us to know that he, irrespectively of what I may have said to the contrary, conceives continuous matter as easily as the space coextensive with it. Now, nothing that is impossible is conceivable, and therefore continuous matter is not impossible.

The argument, if unexceptionable, would fairly meet my objection. But I may be allowed to express my conviction that the fact is not exactly what Professor Norton imagines. The difference between matter and space, with regard to continuity, is such as to allow of no assimilation between them. Dimensions in mere space are mathematical relations only, whilst in matter they would of necessity be physical : moreover space is not a compound made up of a formal infinite multitude of material parts, whilst continuous matter would be such, as it would imply so many distinct parts of matter as can be marked out by endless division : space is vacuity, continuous matter would be fulness : space is only *virtually* continuous, inasmuch as it allows and makes possible continuous motion, whilst matter would be *materially* and *formally* continuous, as is evident. It is useless to enlarge on other such marks of perfect opposition : those mentioned suffice to show the impossibility of *intellectually* conceiving matter and space as possessing any common property. The learned Professor, reconsidering the subject, will, I hope, acknowledge that it is infinitely more difficult " to conceive of an atom of continuous matter, than of the space coextensive with it." Any one who has a true and philosophical notion of space must own that, whatever may be the phantasmagory of our imaginations, it is simply impossible for the intellect to conceive continuous matter as an imitation of continuous space.

But as these considerations might lead us too far into the region of Metaphysics, whither Professor Norton is perhaps little inclined to follow me, I will dismiss the idea of space altogether and fall back on the purport of my objection concerning the continuity of matter. In that objection I said indeed that continuous matter is an impossibility : this was the shortest way of proving that the existence of extended and indivisible atoms was not " an established truth." But to prove this last point it is not necessary to argue from the very impossibility of continuous matter : and therefore, even though it were true that Professor Norton conceives continuous matter as easily as the space coextensive with it, the assumption of the existence of continuous matter in nature would not become " an established truth," but would remain " an arbitrary assumption." I say *arbitrary*, because no science whether speculative or experimental, whether inductive or deductive, whether ancient or modern, affords any

foundation on which to build an argument proving directly or indirectly the truth of the assertion. Let Professor Norton make the attempt : he will then be convinced of what I say.

He adds :

" It is not more difficult to conceive of an indivisible atom acting as a whole upon another atom with a certain energy, than of a mere point acting upon another point, and causing it to change its place, at the same time transferring to a new point all the properties it possesses."

This is his third reply. I doubt whether it has much to do with my objection. Professor Norton is endowed with a very great facility both of *conceiving* everything he likes and of turning his own conceptions into realities : accordingly he conceives " of an indivisible atom acting as a whole," and silently invites us to conclude from this that the existence of continuous matter is an established truth. But is he certain of the fact of his conception ? The action of matter is calculated to cause local motion : it therefore must have intensity and direction. But direction is taken from a mathematical point to a mathematical point. Therefore the action must proceed from a mathematical point, and cannot be conceived to proceed from an extended atom acting *as a whole,* viz. by a single action which is not a resultant of other distinct actions. (See Molecular Mechanics, p. 31.)

Yet I have no need of insisting on this point. My objection was that Professor Norton's " gross matter " being a piece of continuous matter did not exist in nature, and was already proved to be impossible. The right answer would have been to deny either that gross matter is continuous, or that continuous matter has been proved to be impossible. The first alternative would hardly have been consistent with his doctrine (though he will try it in his fifth answer), because it would have stripped matter of its essential epithet " gross : " the second would have obliged him to demolish my proofs (Molecular Mechanics, pp. 27–31): and this he has not done as yet, though it was the best course he could have followed in order to convince his readers of the merit of his theory. Thus my objection still remains unanswered.

His fourth attempt at a solution of the objection is quite original. He says :

" If the occult nature of the force of action of one material point on another be such that the intensity becomes indefinitely small at indefinitely small distances, instead of indefinitely great, as imagined by Professor Bayma, then a collection of an infinite number of material points may form one invariable atom, since the size of

the atom may in every instance be so inappreciable in comparison with the distance between the nearest atoms, that there may never be any inequality of extraneous action on different points of the same atom, imparting different velocities to them, and so tending to break up the continuity of the matter. Besides, we have already seen that no inequality of elementary action, by reason of a difference of distance, is legitimately deducible from Professor Bayma's premisses."

According to this process of reasoning, the nature of the force of action is supposed, for the sake of the cause to be defended, to be *occult*; then, for the sake of the same cause, it is supposed to be such that the intensity becomes indefinitely small at indefinitely small distances : lastly it is inferred that, if this supposition be admitted, " gross matter " will be possible, and will consist of an infinite number of points invariably united. But 1st. The nature of the active powers of matter is by no means so occult as to allow of our inventing any law of action we may be pleased with. 2nd. If the nature of the active powers be occult, then the new law of action imagined by Professor Norton has no ground whatever in the nature of things; and on this account it must be, even in his opinion, " an arbitrary assumption." 3rd. Such a new law is irreconcilable with all molecular and non-molecular science : and on this account it is evidently antiscientific, as it tends " to discard the obvious intimations of nature " and violates the exigencies of the " inductive method." 4th. The same hypothetic law leads to the avowed conclusion that " gross matter " would consist of an *infinite* number of material points : which conclusion implies not only that continuous extension can be made up of unextended points (a proposition openly and notoriously false), but also that a finite being, almost an infinitesimal being, an atom whose size is inappreciable even in comparison with molecular distances, which are themselves almost inappreciable, would consist of an infinity of beings : a proposition, which, I make bold to say, not even Professor Norton, who can *conceive* so many things, will be able to realize. 5th. In the same supposition the material points of which the atom of gross matter is said to be made up would not be *invariably* united, being all compelled to move towards the centre of the atom, and to vibrate to and fro. For those material points are all supposed to be attractive, and actually to attract one another; hence they must approach one another and move all around a central point. And therefore the atom of gross matter would not be invariable. 6th. Let us grant the supposition as possible, however much our reason may revolt against it : even so, gross matter would be only an hypothesis as arbitrary as the new law of action supposed. Is it thus

that Professor Norton proves that his fundamental doctrine is not an " arbitrary assumption " and deserves the name of " established truth " ?

What he says about the " size " of his atom is directed against an argument by which I proved the impossibility of imparting motion to a globule of continuous matter (Molecular Mechanics, pp. 28, 29). Of course, if the size of the atom were smaller than any assignable finite size, my argument would cease to be applicable : but then such an atom would be a mere material point, and gross matter an unreality and an empty name. But, if the size of Professor Norton's atoms remains finite, my argument remains unanswered. For there will be a finite amount of inequality in the intensity of the extraneous actions (especially molecular) on different points of the same atom, imparting to them velocities, the difference between which will be finite and appreciable, and so " tending to break up the continuity of matter."

In my ' Molecular Mechanics ' I had given (pp. 30, 31) a second and, I think, very conclusive proof of the impossibility of continuous matter. The learned Professor says nothing of it, probably because he thought it to consist of what he had previously called " unsubstantialities." Yet the more unsubstantial, the easier should have been the task of its refutation.

As to his saying " We have already seen that no inequality of elementary action, by reason of a difference of distance, is legitimately deducible from Professor Bayma's premisses," the reader needs not to be informed that this is one out of the many gratuitous assertions to which the learned Professor has already accustomed us. He does not even take care to make his assertion credible. He says " We have already seen," and speaks of my " premisses," when no premisses have been quoted by him, and therefore nothing could possibly have been seen to be either legitimately or illegitimately deducible from them.

The fifth answer of my American critic dwells on the word " indivisible," which I understood to convey the notion of absolute indivisibility. He does not admit my interpretation. He says :

" In speaking of atoms of gross matter as indivisible, no other ground was intended to be taken than that each atom was indestructible from any possible action of another atom, and essentially invariable in form. This does not preclude the idea that the atom may be an aggregation of a finite number of material points ; for it may be that the mutual action of two attractive points passes into a repulsion at extremely minute distances, and so that an atom of ordinary matter may be a system of material points in either a statical or dynamical equilibrium. Indivisibility, taken in the only sense

in which the term can properly be used, does not, then, necessarily imply continuity, as maintained by Professor Bayma."

I am sorry that I interpreted the word " indivisible " contrary to the now expressed intention of the learned Professor. Certainly, if " gross matter " is to be considered as an aggregation of a *finite* number of material points being in either a statical or a dynamical equilibrium, I allow that its indivisibility will not imply continuity. Yet if " gross matter " is an aggregation of a finite number of material *points*, why is it to be called *gross* ? And if such material points can be in dynamical equilibrium, how can Professor Norton say that the aggregation of them is " essentially *invariable* in form " ? No aggregation of distinct material points can be *invariable* in form, unless these points be, by some reason or other, immoveable : and an *essential* invariability in the form of the aggregation cannot exist unless those points be *essentially* immoveable. I esteem Professor Norton too much to suppose that he can ever dream of admitting material points essentially immoveable, especially as he holds that these same points may be in a dynamical equilibrium ; for such equilibrium is not essentially inviolable.

This, to my mind, being the case, I was obliged to assume that Professor Norton's atoms " essentially invariable in form " could by no means be considered as an aggregation of a finite number of distinct material points. But when the idea of such an aggregation has been discarded, no other idea remains which can be adopted, except that of continuous matter : and therefore I was compelled to consider the asserted indivisibility and essential invariability of the atom as implying its continuity.

If however Professor Norton now chooses to admit that his atoms of " gross matter " are not continuous matter, I shall be glad to interpret the word " indivisible " according to the meaning now intended by him : yet, even so, my objection will not be nullified ; only instead of being based on the indivisibility of the atom, it will be drawn from the essential invariability of its form. Instead of saying : The atom is a whole indivisible ; therefore it is a piece of continuous matter, I shall say : The atom is a multitude of distinct material points substantially independent ; therefore it cannot be essentially invariable in form.

As to the possibility of a change of attractive into repulsive action at extremely minute distances between primitive *elements* (I say elements, not molecules, whose action is a resultant *variable* according to special laws) I will say nothing in this place, as I have refuted such a view at some length and, I believe, quite sufficiently in my ' Molecular Mechanics ' (pp. 49–52), where I gave also three direct proofs of the contrary. I

mention this in order to make up for Professor Norton's silence on this point.

The sixth and last answer by which the learned Professor strives to weaken the force of my objection is an explanation of the words " spherical in form " applied by him to his atom of gross matter. To say that " all bodies of matter consist of separate indivisible parts called atoms, each of which is conceived to be spherical in form," as Professor Norton says in his 3rd principle, was virtually to say that such atoms were pieces of continuous matter. Such at least was my impression. But he answers :

" The assumption that each atom is spherical in form was adopted merely as the simplest embodiment of the fundamental principles that the action of the atom was equal in all directions, and that the attractive action upon an atom of æther was neutralized at minute distances by the resistance developed at the point of contact. The existence of such a resistance necessarily implies that the elementary parts of the attractive atom, whether finite or infinite in number, act repulsively at very minute distances."

I have already allowed that, when Professor Norton himself explains the meaning attached by him to his own words, I am not entitled to contradict him. It is strange, however, that the expression " spherical in form " which is drawn from geometry, and conveys the clear notion of something geometrical only, should have been in need of an interpretation drawn from mechanical considerations. However this anomaly may be explained, let us take notice first that Professor Norton, in giving this interpretation, reveals to us a new " fundamental principle." The principle is this : " The attractive action upon an atom of æther is neutralized at minute distances by the resistance developed at the point of contact." Is this principle true ? I think not.

I have rigorously proved in my ' Molecular Mechanics ' (I quote my own book for the excellent reason that no other book to my knowledge has yet appeared in which the same subject has been regularly and philosophically developed) that in the true and immediate contact of matter with matter no action is possible (pp. 14, 15). So long as this theorem holds good, I cannot admit that any resistance is developed " at the point of contact " of two atoms. Moreover, if the attractive action of the so-called " gross matter " upon an atom of æther is neutralized " at minute distances," surely repulsion must prevail at such minute distances : but when two atoms are at a minute distance, they are not in contact ; and therefore, if the attractive action is neutralized at minute distances, the resistance developes before the two atoms reach the point of contact : and

therefore the new " fundamental principle," to say the least, is incorrect.

But again, if, according to another view of the learned Professor already noticed, the intensity of action " becomes indefinitely small at indefinitely small distances," we must come to the conclusion that, according to the same view, the intensity of action at the very point of contact will become null. Therefore, if that view is adopted, no resistance will be developed at the point of contact, and the " fundamental principle " will be false, at least hypothetically. Such is the accuracy with which some physicists set down what they call "fundamental principles " and " established truths."

Yet, after all, if an atom of gross matter is more than a material point, the assumption that each atom is spherical in form cannot be the mere embodiment of mechanical principles. An atom which is more than a material point, and possesses " a size " however inappreciable in comparison with the atomic distances, must have a surface : and this surface must have a geometric form either regular or irregular. If it be spherical in form, then it would seem that Professor Norton has tried in vain to discard my geometrical interpretation of his words : whilst, if the geometrical form is not spherical, Professor Norton's own interpretation collapses ; as the action of the atom cannot be conceived " equal in all directions," unless the form of the atom itself be uniformly equal all around, viz. unless the atom be a sphere. The learned author has one means only of avoiding the horns of the dilemma, viz. by allowing that his atoms are systems of discrete material points ; his interpretation of the words " spherical in form " will then be substantially correct, though rather unusual ; and his theory of molecular physics, disembarrassed of *gross* matter and its difficulties, without losing anything worth regretting, will then be able to recommend itself more strongly to a philosophical mind.

In the passage now under examination Professor Norton endeavours also to establish " a change of attractive into repulsive action at very minute distances." As I have fully refuted this view, and the arguments by which Boscovich strove to defend it, in my ' Molecular Mechanics,' I will now only say that the inference drawn by Professor Norton is not legitimate. The existence of a resistance between his atom of gross matter and the atom of æther *does not* necessarily imply that the elementary parts of the attractive atom act repulsively at very minute distances. It implies simply that the so-called gross matter is a dynamical system of elements of which some are attractive and others repulsive, the attractive always attracting, the repulsive always repelling, and the effect of their exertions being *a re-*

sultant attractive or repulsive according as the atoms acted on
are supposed to be placed beyond or within the limits of their
molecular distance of equilibrium. This is the only inference
that can be drawn legitimately from the impenetrability of mo-
lecules.

I might dispense with all remarks on what the writer adds
about a conception which he himself as yet hesitates to adopt.
The idea however is calculated by its novelty and brilliancy
to fascinate a mind devoted to physical speculation, and de-
serves a short notice. The author says :

"But another conception may be formed of the mode of opera-
tion of an atom of gross matter, which involves no other supposition
than that it acts equally outwards in all directions from a centre,
and takes no account of its geometrical extent. This is that *the
effective attraction of the atom for the æther of space is due to the ex-
istence of a repulsion less than would be exerted by the one or more
atoms of æther that would naturally occupy its place.* The result
would be the condensation of an atmosphere of æther around the
atom, without the exertion of any direct attractive force, or of any
additional force of resistance. We may conceive the molecular at-
mosphere of electric æther to originate in a similar way ; but as the
opportunity of examining and testing this idea sufficiently has not
yet been obtained, I shall continue to regard the electric æther as
directly attracted by the atom of gross matter, and that the antago-
nistic force of resistance is furnished by the repulsion of the lumini-
ferous æther condensed around the atom."

This conception, however plausible it may be, is exposed to
many serious objections, which however I am not ready to treat
in this paper, as I must confine myself to the questions already
raised. The least that I can say of this new theory is that it is
quite unnecessary, and that, no matter how much talent may be
spent in building it, it will never be more than an *à priori* as-
sumption ; for in the whole multitude and variety of natural
facts nothing has yet been found which can serve as a basis for
its future demonstration. Professor Norton himself says that
the opportunity of examining and testing this idea *sufficiently*
has not yet been obtained : I rather think that the idea has not
even begun to be tested, and never will, unless the question be
of testing its inadmissibility. For, though there are no facts in
nature supporting the hypothesis, there are facts strongly con-
tradicting it, as for instance molecular cohesion and gravitation.
Moreover, this new hypothesis would not have for its result " the
condensation of an atmosphere of æther around the atom without
the exertion of any direct attractive force," as assumed by Pro-
fessor Norton. The hypothesis that the atom of gross matter
repels less than the æther which would naturally occupy its

place, would lead us to this result only, that the atoms of æther would from every side approach nearer the atom of gross matter, without however becoming closer amongst themselves, that is, without condensation. But, as it is not my intention to discuss an incidental question about which Professor Norton has not yet formed a definite opinion, I will say no more on this subject.

I trust that the reader in the preceding pages will have found sufficient evidence as to whether my criticism on Professor Norton's theory was well founded or not. It only remains for me to answer the objections which he advances against some views put forward in my ' Elements of Molecular Mechanics.' When this has been done, I shall consider the present controversy as closed.

[To be continued.]

L. *On some Reactions of Hydrated Oxide of Ethylene-sodium.* By J. ALFRED WANKLYN, *Professor of Chemistry in the London Institution* *.

AS described in former papers†, hydrated oxide of ethylene-sodium is obtained by allowing metallic sodium to act on ten times its weight of perfectly absolute alcohol and heating the product to rather over 200° C., maintaining it at that temperature so long as alcohol distils off. In this manner a perfectly white product may be obtained having accurately the composition $Na\ C^2 H^5 O$. This substance is hydrated oxide of ethylene-sodium. It is characterized by its reaction with the ethers of the fatty acids and the ether of benzoic acid, with which it gives alcohol and a salt of ethylene-sodium, thus :—

| Hydrated oxide of ethylene-sodium. | Acetic ether. | Acetate of ethylene-sodium. | Alcohol. |

$$\left. \begin{matrix} C^{12} H^4 Na \\ H \end{matrix} \right\} O + \left. \begin{matrix} C^2 H^5 \\ C^2 H^3 O \end{matrix} \right\} O = \left. \begin{matrix} C^2 H^4 Na \\ C^2 H^3 O \end{matrix} \right\} O + \left. \begin{matrix} C^2 H^5 \\ H \end{matrix} \right\} O.$$
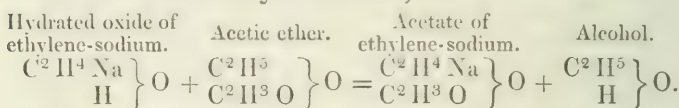
It has already been explained that I do not regard sodium as mono-valent, but as tri-valent in these compounds, the radical ethylene-sodium being

$$\big((C^2\ H^4)''\ Na'''\big)'.$$
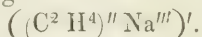
The following reactions, which I have lately studied, tend in favour of this view of the atomicity of sodium.

Sulphuretted Hydrogen and Hydrated Oxide of Ethylene-sodium.

Experiment I. 1·3815 grm. of sodium was dissolved in 15 grms. of absolute alcohol, and the product heated in the oil-bath up to

* Communicated by the Author.
† Phil. Mag. vol. xxxvii. pp. 117 & 175.

above 200° C. The product was then cooled and weighed. Its weight was 4·4645 grms., being a little above the weight of the product completely freed from alcohol. (The theoretical quantity of Na $C^2 H^5 O$ is 4·0814.) It was not considered necessary to drive off the last trace of alcohol in this instance. A current of dry sulphuretted hydrogen was next transmitted over the product, no external heat being applied, but considerable heat being generated by the action of the gas on the substance. After a while the passage of the gas was stopped, and the apparatus with its contents cooled and subsequently weighed. Weight of contents = 5·76 grms. The gas was again transmitted, and again generated heat by its action on the substance. Again the apparatus was cooled and weighed. Weight of contents = 6·175 grms. Again sulphuretted hydrogen was transmitted, this time there being no generation of heat. Weighed again : weight = 6·137 grms., showing that the action of the sulphuretted hydrogen was complete.

This experiment indicates that one molecule of sulphuretted hydrogen combines with one molecule of hydrated oxide of ethylene-sodium.

$$Na''' \left\{ \begin{array}{l} C^2 H^4 \\ O H \end{array} + H^2 S = Na\, C^2 H^7 S\, O. \right.$$
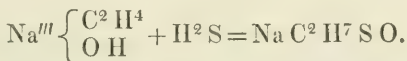
By experiment, 1·3815 grm. of sodium have yielded 6·137 grms. of product ; therefore the percentage of sodium in this product equals 22·51. The theoretical percentage of sodium in Na $C^2 H^7 S O$ is 22·55.

The compound Na $C^2 H^7 S O$, a nearly white solid, the formula of which I write thus,

$$Na''' \left\{ \begin{array}{l} O\, C^2\, H^5 \\ H \\ S\, H, \end{array} \right.$$
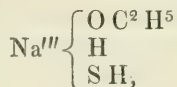
is endowed with considerable stability, having during the process of its formation undergone a considerable spontaneous heating without damage. At 100° C., however, it is gradually decomposed into alcohol and sulph-hydrate of sodium, thus :—

$$2\, Na'' \left\{ \begin{array}{l} O\, C^2\, H^5 \\ H \\ S\, H \end{array} = 2 \left. \begin{array}{l} C^2\, H^5 \\ H \end{array} \right\} O + \left. \begin{array}{l} H^2 \\ Na^2 \end{array} \right\} S^2. \right.$$
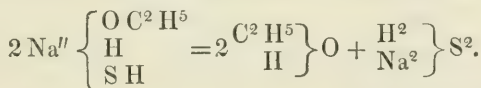
The 6·137 grms. of product, on being heated to 100° C. in the water-bath for some time, lost alcohol (2 grms. of which was condensed and weighed), 3·901 grms. of solid residue remaining. This residue was lastly heated up to 200° C. for some time, when it lost more alcohol and ultimately weighed 3·459 grms. Calculating the percentage of sodium in the 3·459 grms. of pro-

duct which was produced by 1·3815 grm. of sodium, we have

<div style="text-align:center">

Na per cent. $= 39·91$.

Na per cent. $= 41·07$, theory for Na H S.

</div>

Experiment II. In this instance the sulphuretted hydrogen was passed into the hydrated oxide of ethylene-sodium at an elevated temperature, viz. at about 200° C.

3·990 grms. of sodium were dissolved in 60 grms. of absolute alcohol and the product dried at 225° C. Weight $= 11·642$ grms. Calculating the percentage of sodium from these data, Na per cent. $= 34·27$. The theory for Na $C^2 H^5 O$ is Na per cent. $= 33·82$.

Next, a slow stream of sulphuretted hydrogen was passed over the substance at 210° C. for about two hours. During the passage of the gas there distilled out a quantity of liquid which was condensed in a Liebig's condenser having a *narrow* tube.

The distillate was weighed at intervals,

<div style="text-align:center">

(1)   6·5 grms.
(2)   7·3 „
(3)   7·4 „
(4)   7·45 „

</div>

until it became constant. The solid residue was also weighed. Weight of solid residue $= 9·772$ grms.

On referring to the foregoing, it will be seen that this 9·772 grms. of product was made from 3·990 grms. of sodium. Therefore

<div style="text-align:center">

Percentage of sodium found $= 40·83$.

Theory for Na H S . . $= 41·07$.

</div>

The distillate (7·45 grms.) was examined. It was alcohol, and boiled at 77° C. with the utmost constancy quite to dryness. The theoretical yield of alcohol, according to the equation

$$Na\ C^2\ H^7\ S\ O = Na\ H\ S + C^2\ H^6\ O,$$

requires that 7·98 grms. should have been obtained. The quantity 7·45 grms. got in the above experiment is a very good approximation to it, regard being had to the inevitable loss inseparable from a determination of this kind.

Hydrochloric Acid Gas and Hydrated Oxide of Ethylene-sodium.

There is very energetic action between these materials, the generation of heat being so great that it is necessary to cool the apparatus with cold water.

The resulting solid, which is white and amorphous, is tolerably stable.

<div style="text-align:center">

Na employed . . . $= 3·5820$ grms.

Na $C^2 H^5 O$ produced . $= 10·418$ „

HCl $+$ Na $C^2 H^5 O$ given $= 16·148$ „

</div>

From which it follows that the percentage of sodium found in the hydrated oxide of ethylene-sodium $= 34 \cdot 28$. Theory requires $33 \cdot 82$.

The gain of HCl is, in percentage,

100 parts of product gain $159 \cdot 97$ of HCl.

The theory for $Na\,C^2\,H^5\,O + HCl$ is

$$100 \;:\; 158 \cdot 7.$$

The resulting compound, $Na\,C^2\,H^6\,ClO$, breaks up between 100° and 150° into alcohol and hydrochloric acid; after heating the above compound up to 200° C. it weighed $9 \cdot 218$ grms., which contain $3 \cdot 5820$ grms. of sodium.

Chloride of Acetyl and Hydrated Oxide of Ethylene-sodium.

These substances act very energetically, developing much heat. Apparently the compound $Na\,C^2\,H^5\,O,\,C^2\,H^3\,O\,Cl$ is formed. On heating this compound above 100° C., it is resolved into chloride of sodium and acetate of ethyle.

Action of CO^2 on Hydrated Oxide of Ethylene-sodium.

It was a matter of interest to ascertain whether or not carbonic acid and the hydrated oxide of ethylene-sodium would give lactate of soda. Experiment in which the materials were brought together at 180° C. showed that there is no formation of any fixed acid.

$2 \cdot 1$ grms. of sodium were dissolved in absolute alcohol and subsequently heated to 180° C. in a current of dry carbonic acid. In this instance the product was not weighed, but treated with water and subsequently distilled to drive off the aqueous alcohol. The distillate, consisting of weak alcohol, measured $14 \cdot 5$ cubic centims., and its specific gravity at 16° C. was $0 \cdot 9670$. It contained therefore $3 \cdot 48$ grms. of absolute alcohol. The residue left after the distillation of the weak alcohol was titrated with standard acid. It saturated $91 \cdot 5$ cubic centims. of normal acid, and therefore contained $2 \cdot 1045$ grms. of sodium uncombined with any permanent acid. During the progress of the titration the observation was made that the sodium was fully saturated with carbonic acid. Collecting these data,

$$
\begin{array}{llll}
\text{Na taken} & . \quad . \quad . \quad . \quad . & = 2 \cdot 10 \text{ grms.} \\
\text{Na found unneutralized} & . & = 2 \cdot 1045 \text{ ,,} \\
\text{Alcohol obtained} & . \quad . \quad . & = 3 \cdot 48 \text{ ,,}
\end{array}
$$

No special care was bestowed on this determination of alcohol. The close correspondence between the unneutralized sodium after the experiment and that taken for experiment is demonstration of the non-formation of any lactate of soda during the action

of CO^2 on $Na\,C^2\,H^5\,O$ at high temperatures. Carbonic acid therefore only forms carbonate of ethylene-sodium when made to act on the hydrated oxide of ethylene-sodium.

Action of Oxide of Carbon, CO.

It is stated in many of the chemical handbooks that carbonic oxide and ethylate of sodium give propionate of soda. What the authority may be for this statement I have never been able to ascertain. Some years ago several chemists, including myself, made the crystals of so-called ethylate of sodium and subjected them to the action of oxide of carbon at 100° C. We were unable to observe any formation of propionate of soda. Another statement respecting the action of these substances is that they give a pseudo-propionate of soda, which on treatment with water gave formiate of soda and alcohol.

I have therefore studied the action of carbonic oxide on the hydrated oxide of ethylene-sodium with the special object of ascertaining whether or not there was formation of a soda-salt.

I employed 2·032 grms. of sodium, which was dissolved in absolute alcohol, and the resulting product heated up to 220° C. for some time in a current of dry carbonic oxide. The weight of the product after this exposure to the action of the gas was 5·898 grms., being only the unaltered hydrated oxide of ethylene-sodium. Calculating the percentage of sodium in the product from these data, we have Na per cent. = 34·45. The theory for hydrated oxide of ethylene-sodium is Na per cent. = 33·82. Therefore no absorption of CO takes place even at 220° C. In further confirmation of this result, water was added to the product and a titration made with normal acid. It saturated 82·5 cubic centims. of normal acid, and therefore contained 1·897 grm. of sodium in the caustic state—a result which confirms the statement that there is no action between carbonic oxide and hydrated oxide of ethylene-sodium.

Action of Ammonia.

A quantitative experiment showed that at 240° C. there is no action between hydrated oxide of ethylene-sodium and ammonia.

The following new compounds have been described in this paper:—

$$Na''' \begin{cases} O\,C^2\,H^5 \\ H \\ S\,H. \end{cases}$$

$$Na''' \begin{cases} O\,C^2\,H^5 \\ H \\ Cl. \end{cases}$$

$$\text{Na}'''\begin{cases} \text{O C}^2\,\text{H}^5 \\ \text{C}^2\,\text{H}^3\,\text{O} \\ \text{Cl}. \end{cases}$$
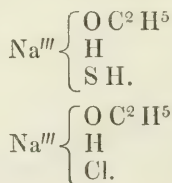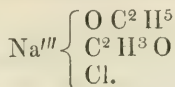
The large amount of heat evolved during the formation of these compounds proves them to be genuine cases of chemical combination, and not merely instances of juxtaposition of molecules.

LI. *On the Mechanical Impossibility of the Descent of Glaciers by their Weight only. By* Henry Moseley, *M.A., Canon of Bristol, F.R.S., Instit. Imp. Sc. Paris, Corresp.**

THE following is the mathematical discussion of the conditions of the descent of a glacier of unlimited length and uniform rectangular section and slope, from which I have deduced the conclusion that it is mechanically impossible the Mer de Glace should descend as it does by its weight only, in a paper of which an abstract was read before the Royal Society on the 7th of January, and published in the Proceedings of the Society and in your Magazine for March.

Let A B C D represent part of the surface of a glacier of
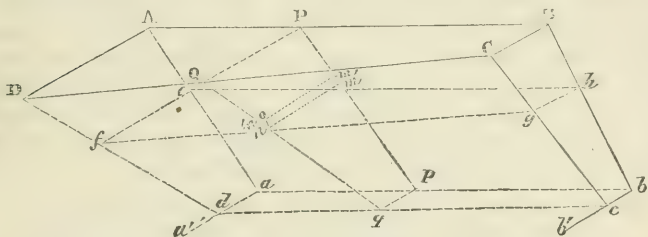
uniform slope and uniform width and depth, A D being a portion of the central line of the surface, and B C of the edge, and A B at right angles to A D and B C. Also let *a b c d* be a part of the floor of the glacier, and A B *b a* a section perpendicular to the floor and the surface of the glacier and to the direction of its motion, and let A *a d* D and B *b c* C be planes perpendicular to the surface and floor, but parallel to the direction of the motion of the glacier. Let A D be the space traversed by a particle in the central line of the surface in any given time, B C that described by a particle in the edge of the surface in the same time, and *a d* and *b c* spaces similarly described by points in the centre and edge of the bottom of the glacier. Imagine the trapezoidal solid A *c* to be intersected by an infinite number of planes pa-

* Communicated by the Author.

rallel to A *a d* D, and equidistant from one another, and let P *p q* Q be one of these planes. Imagine it, too, to be similarly intersected by planes parallel to A B C D. Let the intersections of these planes *m n* be the edge of one of the rectangular strips into which the solid will thus be divided. If we suppose the strip *m n* to be prolonged indefinitely both ways, every particle of ice in it will be moving with the same velocity, the glacier being supposed to be of indefinite length and uniform dimensions and slope. The strip *m n* will therefore sustain no pressure downwards from its upward prolongation, nor resistance upwards from its downward prolongation; and supposing the weight of the glacier to be the only force tending to bring it down, the *work* of the weight of every particle of ice in *m n* must be equal to the work of the resistances which oppose themselves to its uniform descent; and since all the particles of *m n* may be assumed to have equal weights and to sustain equal resistances, what is true of a single particle is true of the whole elementary strip of ice *m n*. The resistances to the descent of *m n* are, first, the *shear* of its surfaces on the surfaces of similar contiguous strips above and below and on each side of it, and, secondly, its friction on the strips below and above it. Besides these resistances, to which its elements, such as *m n*, are subjected by reason of the different velocities with which its parts move, and which consume the work of its weight internally, there is the work necessary to move the whole solid A *c* over its floor *a b c d*, and to overcome its adhesion to its side B *b c* C. Let it be supposed that the floor and sides of the channel of the glacier are so rough that it is necessary to *shear* the ice over them.

Let μ = the *shear* of the ice in lbs. per unit of surface.
ι = inclination of floor of glacier to horizon.
f = coefficient of friction of ice upon ice.
w = weight of ice per solid unit.

$$AB = a, \quad Aa = b, \quad bp = x, \quad pm = y,$$
$$AD = \alpha, \quad BC = \beta, \quad bc = \gamma, \quad ad = \frac{\alpha\gamma}{\beta}.$$

$U \sin \iota$ = whole work of the *weight* of solid A *c* in unit of time.
$U_1 \mu$ = whole work of internal *shear* of solid A *c* in unit of time.
$U_2 \mu$ = work of external shear of bottom *a b c d* in unit of time.
$U_3 \mu$ = work of external shear of side B *b c* C in unit of time.
$U_4 f \cos \iota$ = work of internal friction of solid A *c* in unit of time.
$U_5 f \cos \iota$ = work of friction on floor of channel in do.

Then, by the principle of virtual velocities,

$$U \sin \iota = U_1 \mu + U_2 \mu + U_3 \mu + U_4 f \cos \iota + U_5 f \cos \iota, \quad . \quad (1')$$

$$\mu = \frac{U \sin \iota - (U_4 + U_5) f \cos \iota}{U_1 + U_2 + U_3}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$PQ = BC + \frac{BP}{BA}(AD - BC) = \beta + \frac{x}{a}(\beta - \alpha) = \frac{a\beta + x(\alpha - \beta)}{a}.$$

Let the motion of the bottom of the glacier in the centre be supposed to bear the same proportion to the motion at the top in the centre that that of the bottom of the sides does to the top at the sides. Or let $\frac{ad}{AD} = \frac{bc}{BC}$; whence $ad = \frac{\alpha\gamma}{\beta}$;

$$\therefore pq = bc + \frac{bp}{ba}(ad - bc) = \gamma + \frac{x}{a}\left(\frac{\alpha\gamma}{\beta} - \gamma\right) = \frac{\gamma}{\beta a}\left[a\beta + x(\alpha - \beta)\right];$$

$$\therefore PQ - pq = \frac{a\beta + x(\alpha - \beta)}{a} - \frac{\gamma}{a\beta}\left[a\beta + x(\alpha - \beta)\right]$$

$$= \left(1 - \frac{\gamma}{\beta}\right)\frac{a\beta + x(\alpha - \beta)}{a};$$

$$\therefore mn = pq + \frac{Pm}{Pp}(PQ - pq) = \frac{\gamma}{\beta a}\left[a\beta + x(\alpha - \beta)\right]$$

$$+ \frac{y}{b}\left(1 - \frac{\gamma}{\beta}\right)\frac{a\beta + x(\alpha - \beta)}{a},$$

$$mn = \frac{a\beta + x(\alpha - \beta)}{a}\left[\frac{\gamma}{\beta} + \frac{y}{b}\left(1 - \frac{\gamma}{\beta}\right)\right]$$

$$= \frac{\left[a\beta + x(\alpha - \beta)\right]\left[b\gamma + y(\beta - \gamma)\right]}{ab\beta}.$$

Weight of prism $m\,n = w \cdot \overline{mn}\, dx\, dy.$

Work of the weights of the particles descending $m\,n$ while m descends from m to n = weight of prism $\times \frac{1}{2}\overline{mn}\sin \iota$;

$$\therefore dU\sin\iota = \frac{1}{2}w\,\overline{mn}^2\, dx\, dy \sin\iota; \quad . \quad . \quad . \quad . \quad (2')$$

$$\therefore dU = \frac{1}{2}w\frac{\left[a\beta + x(\alpha - \beta)\right]^2\left[b\gamma + y(\beta - \gamma)\right]^2}{a^2 b^2 \beta^2}\, dx\, dy,$$

$$U = \frac{1}{2}w\int_0^a\int_0^b\frac{\left[a\beta + x(\alpha - \beta)\right]^2\left[b\gamma + y(\beta - \gamma)\right]^2}{a^2 b^2 \beta^2}\, dx\, dy,$$

$$U = \frac{wab(\alpha^3 - \beta^3)(\beta^3 - \gamma^3)}{18\beta^2(\alpha - \beta)(\beta - \gamma)},$$

$$U = \frac{wab}{18\beta^2}(\alpha^2 + \alpha\beta + \beta^2)(\beta^2 + \beta\gamma + \gamma^2). \quad . \quad . \quad . \quad (2)$$

Let $m'\,n'$ be the elementary prism which rests immediately on $m\,n$. Draw $o\,n$ perpendicular to $m'\,n'$. Then is $o\,n'$ the distance through which $m'\,n'$ is sheared over $m\,n$. Moreover the resistance to this shearing is the constant quantity $\mu\,\overline{mn}\,dx$;

$\therefore$ work of the horizontal shear of $mn = \mu\,.\,\overline{mn}\,.\,\overline{on'}\,.\,dx$.

But $o\,n' = m'\,n' - m\,n = \dfrac{d\,\overline{mn}}{dy}\,dy$;

$\therefore$ work of horizontal shear of $m\,n$

$$= \mu\,.\,\overline{mn}\,\frac{d\,\overline{mn}}{dy}\,dx\,dy. \qquad . \quad . \quad . \quad . \quad . \quad . \quad (3')$$

But $m\,n = \dfrac{[a\beta + x(\alpha - \beta)]\,[b\gamma + y(\beta - \gamma)]}{ab\beta}$;

$$\therefore \frac{d\,\overline{mn}}{dy} = \frac{(\beta - \gamma)\,[a\beta + x(\alpha - \beta)]}{ab\beta} ;$$

$\therefore$ work of horizontal shear of $m\,n$

$$= \frac{\mu(\beta - \gamma)}{a^2 b^2 \beta^2}\,[a\beta + x(\alpha - \beta)]^2 [b\gamma + y(\beta - \gamma)]\,dx\,dy.$$

Whole work of internal horizontal shear of trapezoid A c

$$= \frac{\mu(\beta - \gamma)}{a^2 b^2 \beta^2} \int_0^a \int_0^b [a\beta + x(\alpha - \beta)]^2 [b\gamma + y(\beta - \gamma)]\,dx\,dy$$

$$= \frac{\mu a(\alpha^3 - \beta^3)(\beta^2 - \gamma^2)}{6\beta^2(\alpha - \beta)}.$$

In the same manner it may be shown that work of vertical shear of $m\,n$

$$= \mu\,\overline{mn}\,\frac{d\,\overline{mn}}{dx}\,dx\,dy \qquad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4')$$

$$= \frac{(\alpha - \beta)}{a^2 b^2 \beta^2}\,[b\gamma + y(\beta - \gamma)]\,[a\beta + x(\alpha - \beta)]\,dx\,dy.$$

Whole work of internal vertical shear of trapezoid A c

$$= \frac{\mu(\alpha - \beta)}{a^2 b^2 \beta^2} \int_0^a \int_0^b [b\gamma + y(\beta - \gamma)]^2 [a\beta + x(\alpha - \beta)]\,dx\,dy$$

$$= \frac{\mu b(\alpha^2 - \beta^2)(\beta^3 - \gamma^3)}{6\beta^2(\beta - \gamma)} ;$$

∴. whole work of internal shear of trapezoid A *c*

$$= U_1\mu = \frac{\mu}{6\beta^2}\left[\frac{a(\alpha^3-\beta^3)(\beta^2-\gamma^2)}{(\alpha-\beta)} + \frac{b(\beta^3-\gamma^3)(\alpha^2-\beta^2)}{(\beta-\gamma)}\right],$$

$$U_1 = \frac{1}{6\beta^2}[a(\beta^2-\gamma^2)(\alpha^2+\alpha\beta+\beta^2)+b(\alpha^2-\beta^2)(\beta^2+\beta\gamma+\gamma^2)]. \quad (3)$$

If we suppose the bottom *a b c d* of the trapezoid A *c* to be divided into an infinite number of equal elements, they will have been sheared over different parallel distances as the trapezoid moves into its present position, and the work of the shear of each element will be represented by the shear multiplied by the corresponding distance, so that the whole work of the shear of this displacement will be represented by the sum of these products. But the sum of these products is equal to the sum of the shears (that is, the whole shear of the base of the trapezoid) multiplied by the distance which the centre of gravity of the base has traversed in the act of displacement.

This distance is represented by

$$\frac{a[\gamma^2+\gamma\gamma'+\gamma'^2]}{6\dfrac{a(\gamma+\gamma')}{2}}, \text{ if } \gamma'=a\,d.$$

Also the whole shear of the base

$$= \frac{\mu a(\gamma+\gamma')}{2};$$

∴. work of shear of base

$$= \mu U_2 = \frac{\mu a}{6}(\gamma^2+\gamma\gamma'+\gamma'^2).$$

But $\gamma' = \dfrac{\gamma\alpha}{\beta}$,

$$\therefore\ U_2 = \frac{a}{6}\left(\gamma^2+\frac{\gamma^2\alpha}{\beta}+\frac{\gamma^2\alpha^2}{\beta^2}\right),$$

$$U_2 = \frac{a\gamma^2}{6\beta^2}(\beta^2+\alpha\beta+\alpha^2). \quad . \quad . \quad . \quad . \quad . \quad (4)$$

In the same manner it may be shown that work of shear of the side B *b c* C of the trapezoid

$$\mu U_3 = \frac{\mu b}{6}(\beta^2+\gamma\beta+\gamma^2),$$

$$\therefore\ U_3 = \frac{b}{6}(\beta^2+\beta\gamma+\gamma^2). \quad . \quad . \quad . \quad . \quad . \quad (5)$$

Weight of vertical lamina $P\,m\,n\,Q = \frac{1}{2}w(PQ + mn)\,\overline{Pm}\,dx.$

Normal pressure of weight on $\overline{mn}$ = weight $\times \cos\iota.$

Friction of weight of $P\,m\,n\,Q$ on $\overline{mn} = f$ weight $\times \cos\iota.$

Distance traversed by $m'\,n'$ over $m\,n = \dfrac{d\,\overline{mn}}{dy}\,dy.$

Work of friction of $P\,m\,n\,Q$ on $m\,n$

$$= \tfrac{1}{2}\,wf\cos\iota(PQ + mn)\,\overline{Pm}\,\frac{d\,\overline{mn}}{dy}\,dx\,dy. \quad . \quad . \quad . \quad (5')$$

$$PQ + mn = \frac{a\beta + x(\alpha - \beta)}{a} + \frac{[a\beta + x(\alpha - \beta)]\,[b\gamma + y(\beta - \gamma)]}{ab\beta}$$

$$= \frac{[a\beta + x(\alpha - \beta)]\,[(\beta + \gamma)b + y(\beta - \gamma)]}{ab\beta},$$

$$\frac{d\,\overline{mn}}{dy} = \frac{(\beta - \gamma)\,[a\beta + x(\alpha - \beta)]}{ab\beta};$$

$\therefore$ work of friction of $PmnQ$ on $\overline{mn}$

$$= \frac{wf\cos\iota(\beta - \gamma)}{2a^2b^2\beta^2}\,[a\beta + x(\alpha - \beta)]^2\,[(\beta + \gamma)b + (\beta - \gamma)y]\,(b - y)\,dx\,dy\,;$$

$$\therefore U_4f\cos\iota = \frac{wf\cos\iota(\beta - \gamma)}{2a^2b^2\beta^2}\int_0^a\int_0^b [a\beta + x(\alpha - \beta)]^2[(\beta + \gamma)b$$
$$+ (\beta - \gamma)y]\,(b - y)\,dx\,dy,$$

$$U_4f\cos\iota = \frac{wf(\beta - \gamma)\cos\iota}{2a^2b^2\beta^2}\Big[\tfrac{1}{3}(a^3\alpha^3 - a^3\beta^3)\,[\tfrac{1}{2}(\beta + \gamma)b^3$$
$$+ \tfrac{1}{6}(\beta - \gamma)b^3]\Big],$$

$$U_4f\cos\iota = \frac{wabf(\beta - \gamma)(\alpha^3 - \beta^3)(2\beta + \gamma)}{18\beta^2(\alpha - \beta)}\cos\iota,$$

$$U_4f\cos\iota = \frac{wabf(\beta - \gamma)(2\beta + \gamma)(\alpha^2 + \alpha\beta + \beta^2)}{18\beta^2}\cos\iota. \quad . \quad . \quad (6)$$

Work of friction of lamina $P\,p\,q\,Q$ on $p\,q$

$$= \text{weight } PpqQ \times \cos\iota \times f \times \tfrac{1}{2}\,\overline{pq},$$

weight $PpqQ = w\,\overline{Pp}\,.\,\tfrac{1}{2}\,\overline{PQ + pq}\,dx$

$$= \tfrac{1}{2}\frac{wb\,[a\beta + x(\alpha - \beta)]\,(\beta + \gamma)}{a\beta}\,dx,$$

$$\tfrac{1}{2}pq = \frac{\gamma}{2a}\,[a\beta + x(\alpha - \beta)].$$

Work of friction of P $p\,q$ Q on $p\,q$

$$= d\mathrm{U}_5 f \cos \iota = \frac{wb\gamma f(\beta+\gamma)\,[a\beta + x(\alpha-\beta)]^2 \cos \iota}{4a^2\beta^2}\,dx,$$

$$\mathrm{U}_5 f \cos \iota = \frac{wb\gamma f(\beta+\gamma)\cos \iota}{4a^2\beta^2} \int_0^a [a\beta + x(\alpha-\beta)]^2 dx,$$

$$\mathrm{U}_5 f \cos \iota = \frac{wb\gamma f(\beta+\gamma)\cos \iota}{4a^2\beta^2}\left[\frac{\tfrac{1}{3}(a^3\alpha^3 - a^3\beta^3)}{\alpha-\beta}\right],$$

$$\left.\begin{aligned}
\mathrm{U}_5 f \cos \iota &= \frac{wab\gamma f(\beta+\gamma)(\alpha^3-\beta^3)}{12\beta^2(\alpha-\beta)}\cos \iota\\
&= \frac{wab\gamma f(\beta+\gamma)(\alpha^2+\alpha\beta+\beta^2)}{12\beta^2}\cos \iota
\end{aligned}\right\} \quad . \quad (7)$$

Substituting these values of U, U_1, U_2, U_3, U_4, U_5 in equation (1) and reducing,

$$\mu = \tfrac{1}{6} wab(\alpha^2+\alpha\beta+\beta^2)\frac{2(\beta^2+\beta\gamma)\sin \iota - (4\beta^2+\beta\gamma+\gamma^2)f\cos \iota}{a\beta^2(\alpha^2+\alpha\beta+\beta^2)+ba^2(\beta^2+\beta\gamma+\gamma^2)}. \,(8)$$

By the measurements of Professor Tyndall on the Mer de Glace,

$$*a = 699, \quad *b = 136\cdot8, \quad *\alpha = 1\cdot9375, \quad \dagger\beta = \cdot5416',$$
$$\dagger\gamma = \cdot2133', \quad \ddagger w = 62\cdot5, \quad \S f = \cdot022, \quad \iota = 4^\circ\,52'.$$

Substituting which values, we obtain

Equation (2), $\quad$ U $\sin \iota = 2657583 \sin \iota.$
Equation (3), $\quad\quad \mathrm{U}_1 = \quad 89836.$
Equation (4), $\quad\quad \mathrm{U}_2 = \quad 13263.$
Equation (5), $\quad\quad \mathrm{U}_3 = \quad\;\; 1492.$
Equation (6), $\mathrm{U}_4 f \cos \iota = \quad 54028 \cos \iota.$
Equation (7), $\mathrm{U}_5 f \cos \iota = \quad 30656 \cos \iota.$
Equation (1),

$$\mu = \frac{\mathrm{U}\sin \iota - (\mathrm{U}_4 + \mathrm{U}_5)f\cos \iota}{\mathrm{U}_1 + \mathrm{U}_2 + \mathrm{U}_3} = \frac{2657583 \sin \iota - 84684\cos\iota}{104591}. \quad (9)$$

In the case of the Mer de Glace, $\iota = 4^\circ\,52'$;

$$\therefore\; \mu = \frac{225458 - 84380}{104591} = 1\cdot3489. \quad . \quad (10)$$

* Phil. Trans. vol. cxlix. pp. 265 & 266.
† Tyndall's 'Glaciers of the Alps,' p. 292.
‡ The specific gravity of glacier-ice is assumed equal to that of water.
§ By the experiments of Mr. G. Rennie.

Now the unit of shear in ice is at the least 75* lbs. per square inch. It is therefore mechanically impossible that the Mer de Glace should descend by its weight only. Some other force in addition to its weight must act upon it to cause it to descend. This force must, moreover, be such as would produce those molecular displacements and strains which are observed actually to take place in glacier-ice, and must therefore be, as its weight is, present to every part of the glacier, but more than thirty-four times as great.

LII. *On the Uniform Motion of an Imperfect Fluid.* By HENRY MOSELEY, *F.R.S. &c.*†

BY a perfect fluid I understand a fluid to the displacement of whose parts among one another no resistance is opposed either of shearing or friction ; and by an imperfect fluid one to the displacement of whose parts among one another there is opposed the resistance of shearing but not of friction. All fluids are perhaps in this sense imperfect fluids.

Let A *a b* B represent half a transverse section of an im-

perfect fluid descending in a straight channel, having a uniform slope and a rectangular section everywhere of the same dimensions. Conceive the plane surface D *d c* C to be replaced by a curved surface, and let A D *d c* C B (bounded in front by this
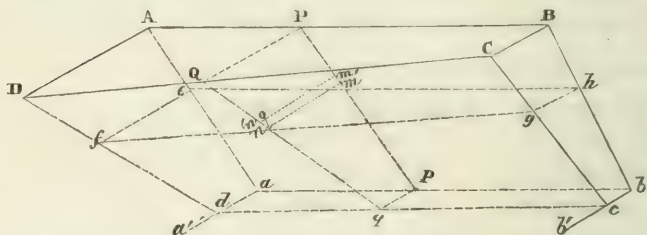
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* The unit of shear in soft putty is from 1 lb. to 3 lbs. per square inch. The Mer de Glace would therefore descend by its weight only if it were of the same degree of hardness as that substance and of the same specific gravity. Although the resistance to shearing in ice is great enough to stop effectually the descent of a glacier by its weight, yet it is probably less than the resistance to shearing of any other solid. The unit of shear in wrought iron is 50,000 lbs. per square inch ; in cast iron 27,000 lbs. ; in oak 2300 lbs. ; in ash and elm 1400 lbs. ; in larch 970 to 1700 lbs. ; in spruce 600 lbs. ; and in red pine from 500 to 800 lbs.

† Communicated by the Author.

curved surface) represent the mass of fluid which passes through the section A *a b* B in a unit of time.

Representing by Σu the work of any system of forces other than the weight of the fluid which tends (with its weight) to cause it to descend, and adopting in other respects the same symbols as in the last article and the same reasoning, we have by equation $(1')$, observing that in this case $f=0$,

$$\mu(U_1 + U_2 + U_3) = U \sin \iota + \Sigma u \; ;$$

where

$\mu =$ the unit of shear.

$U \sin \iota =$ the work of the weight of the fluid which passes through the transverse section A *a b* B in a unit of time.

$U_1\mu =$ the *internal* work of the shear of the same fluid.

$U_2\mu =$ the work of the shear of the same fluid on the bottom of its channel.

$U_3\mu =$ the work of the shear of the same fluid on the sides of its channel.

By equation $(2')$,

$$U = \tfrac{1}{2} w \iint \overline{mn}^2 \, dx \, dy = \tfrac{1}{2} w \iint z^2 \, dx \, dy \; ;$$

by equations $(3', 4')$,

$$\mu U_1 = \mu \iint \overline{mn} \frac{d\overline{mn}}{dy} \, dx \, dy + \mu \iint \overline{mn} \frac{d\overline{mn}}{dx} \, dx \, dy,$$

$$\mu U_1 = \mu \iint \left(z \frac{dz}{dy} + z \frac{dz}{dx} \right) dx \, dy,$$

$$\mu U_2 = \tfrac{1}{2} \mu \int z_1^2 \, dx,$$

where z_1 is any value of z for which $y=0$, or which is measured on the bottom of the channel

$$\mu U_3 = \tfrac{1}{2} \mu \int z_2^2 \, dy,$$

where z_2 is any value of z for which $x=0$, or which is measured on the sides of the channel ;

$$\therefore \; \mu \iint \left(z \frac{dz}{dx} + z \frac{dz}{dy} \right) dx \, dy + \tfrac{1}{2} \mu \int z_1^2 \, dx + \tfrac{1}{2} \mu \int z_2^2 \, dy$$

$$= \tfrac{1}{2} w \sin \iota \iint z^2 \, dx \, dy + \Sigma u.$$

Differentiating successively in respect to x and y, and observing that $\int z_1^2 dx$ is not a function of y, nor $\int z_2^2 dy$ of x,

$$\mu\left(z\frac{dz}{dx} + z\frac{dz}{dy}\right) = \tfrac{1}{2} w \sin\iota\, z^2 + \Sigma\left(\frac{du}{dx\,dy}\right).$$

If the fluid descends by its weight only, $\dfrac{du}{dz\,dy} = 0$;

$$\therefore \mu\left(\frac{dz}{dx} + \frac{dz}{dy}\right) = \tfrac{1}{2} wz \sin\iota.$$

Let $m = \dfrac{w\sin\iota}{2\mu}$,

$$\therefore \frac{dz}{dx} + \frac{dz}{dy} = mz.$$

The general primitive of this partial differential equation is readily found by the method of Lagrange to be

$$x = C\epsilon^{mx + \phi(y-x)} ;$$

or, since x and y enter symmetrically,

$$z = C\epsilon^{my + \phi(x-y)},$$

where ϕ is an arbitrary function of $(x-y)$. A complete primitive is found by the method of Charpit to be

$$mz = A\epsilon^{mx} + B\epsilon^{my}.$$

The whole volume Q of the fluid discharged in a unit of time through a rectangular portion of the transverse section represented by xy is

$$Q = \int_0^x\int_0^y z\,dx\,dy = \frac{1}{m}\int_0^x\int_0^y (A\epsilon^{mx} + B\epsilon^{my})dx\,dy,$$

$$\therefore Q = \frac{1}{m^2}\{Ay(\epsilon^{mx}-1) + Bx(\epsilon^{my}-1)\}.$$

Or, substituting the value of m,

$$z = \frac{2\mu}{w\sin\iota}\left\{A\epsilon^{\frac{w\sin\iota}{2\mu}x} + B\epsilon^{\frac{w\sin\iota}{2\mu}y}\right\}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$Q = \left(\frac{2\mu}{w\sin\iota}\right)^2\left\{Ay(\epsilon^{\frac{w\sin\iota}{2\mu}x}-1) + Bx(\epsilon^{\frac{w\sin\iota}{2\mu}y}-1)\right\} . \quad . \quad (2)$$

In equation (1) z represents the velocity of the stream at the point xy. This equation only applies to one-half the section—from one side to the centre. The whole discharge is therefore to be determined by substituting a for x and b for y in equation (2) and taking twice the result.

The values of the constants μ, A, B may be determined by observing the quantities Q_1, Q_2, Q_3 of fluid discharged through the same channel with three different supplies, and therefore three different depths of the fluid in the channel or values of b. These constants being determined, the velocities z_1 and z_2 at the bottom and sides are known from equation (1). That equation shows the velocity to increase from the bottom to the surface and from the sides to the centre, which corresponds with observation.

LIII. *Note on a new Continued Fraction applicable to the Quadrature of the Circle.* By J. J. SYLVESTER*.

IN a recent note inserted by the author in the Philosophical Magazine it was virtually shown, and indeed becomes almost self-evident *as soon as stated*, that the equation $u_{x+1} = \dfrac{u_x}{x} + u_{x-1}$ possesses two particular integrals, α_x, β_x, which are the products of x terms of the respective progressions

$$[1, \quad 1, \quad \tfrac{3}{2}, \quad 1, \quad \tfrac{5}{4}, \quad 1, \quad \tfrac{7}{6}, \dots];$$
$$[1, \quad \tfrac{2}{1}, \quad 1, \quad \tfrac{4}{3}, \quad 1, \quad \tfrac{6}{5}, \quad 1, \dots].$$

Now any continued fraction whose partial quotients are $\dfrac{1}{k}$, $\dfrac{1}{k+1}$, ..., $\dfrac{1}{x}$ will be equal to the ratio of some two particular values of u_x in the above equation, *i. e.* of two linear functions of α_x, β_x; and in especial when $k=1$ it will be found very easily that this fraction is $\dfrac{\beta_x - \alpha_x}{\alpha_x}$.

But, on supposing x infinite, $\dfrac{\beta_x}{\alpha_x}$ becomes equal to the well-known factorial expression for $\dfrac{\pi}{2}$, viz. $\tfrac{2}{1} \cdot \tfrac{2}{3} \cdot \tfrac{4}{3} \cdot \tfrac{4}{5} \cdot \tfrac{6}{7} \dots$ Hence we may deduce the following value for $\dfrac{\pi}{2}$ under the form of a continued fraction, viz.

$$\frac{\pi}{2} = 1 + \frac{1}{1} + \frac{1}{2^{-1}} + \frac{1}{3^{-1}} + \frac{1}{4^{-1}} \; ad \; infinitum.$$

Reverting to pure integers, the above equality may be written

as follows,

$$\frac{\pi}{2} = 1 + \frac{1}{1} + \frac{2}{1} + \frac{6}{1} + \frac{12}{1} + \frac{20}{1} \ ad \ infinitum,$$

the denominators of the partial fractions being all units, and the denominators (after the first) the doubles of the natural series of triangular numbers $1, 3, 6, 10 \ldots$. This is obviously the simplest form of continued fraction for π that can be given, and yet, strange to say, has not, I believe, before been observed. Truly wonders never cease !

At first sight it might seem as if the above-stated continued fraction were incapable of teaching anything that cannot be got direct out of the Wallisian representation itself that has become transformed into it. Thus, *ex. gr.*, the convergent

$$1 + \frac{1}{1+} \quad \frac{2}{1+} \quad \frac{6}{1+} \quad \frac{12}{1}, \ i. \ e. \ \frac{64}{45},$$

is identical with the corresponding factorial product $\dfrac{2 \cdot 2 \cdot 4 \cdot 4}{1 \cdot 3 \cdot 3 \cdot 5}$.

But I think a substantial difference does arise in favour of the continued fraction form, inasmuch as it indicates a certain obvious correction to be applied in order that the convergence may become more exact. For if we call

$$\frac{n(n+1)}{1+} \quad \frac{(n+1)(n+2)}{1+} \ldots \ ad \ infinitum \ u_n,$$

we have $u_n = \dfrac{n^2 + n}{1 + u_{n+1}}$. This shows that u_n cannot remain finite when n becomes infinite; for then u_{n+1} would also be finite, and consequently u^n would be a finite fraction of infinity, which is a contradiction in terms.

Hence ultimately

$$n_n . \ u_{n+1} = n^2 + n, \ \text{i. e.} \ u_n = n,$$

or, in other words,

$$\frac{1}{n^{-1}+} \quad \frac{1}{(n+1)^{-1}+} \quad \frac{1}{(n+2)^{-1}+} \ldots \ ad \ infinitum,$$

converges (and, it may be shown, always in an *ascending* direction) towards unity as its limit when n converges towards infinity. Thus we may write when n is very great,

$$\frac{\pi}{2} = 1 + \frac{1}{1+} \; \frac{2}{1+} \; \frac{6}{1+} \cdots + \frac{n-n^2}{1+n} *.$$

Ex. gr., when $n=4$, $\frac{\pi}{2}$ approximately equals

$$1 + \frac{1}{1+} \; \frac{2}{1+} \; \frac{6}{1+} \; \frac{12}{1+4}, \; i.\,e. = \frac{128}{81}, \text{ or } 1\cdot 5802,$$

and. when $n=5$, will be found to be $\frac{352}{225}$ or $1\cdot 5644$. The un-corrected convergent corresponding to the former of these is, as we have seen, $\frac{64}{45}$, or $1\cdot 4222$; and the next is $\frac{384}{225}$, or $1\cdot 7056$, the true value of $\frac{\pi}{2}$ being $1\cdot 5708$. The errors given by the uncorrected factorial values are $\cdot 1486$ and $\cdot 1348$ respectively (of course with opposite signs), whereas the errors corresponding to the corrected values are only $\cdot 0094$ and $\cdot 0064$; the approximation being thus more than fifteen and twenty-one times bettered for the fourth and fifth convergents respectively by aid of the correction.

Athenæum Club,
April 1869.

LIV. *On two remarkable Resultants arising out of the Theory of Rectifiable Compound Logarithmic Waves.* By J. J. SYL-VESTER†.

THE fruitful investigations in which I have been for some time past engaged concerning reducible cyclodes and recti-fiable compound logarithmic waves have led me *inter alia* to notice a problem of elimination which from its elegance and peculiarity is, I think, worthy of being offered in a detached form to the Philosophical Magazine.

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* This comes to the same thing as saying that for the purposes of cal-culation the continued fraction should be always considered as ending with a numerator, 1, and not with a denominator, $\frac{1}{k}$. *Ex. gr.* $1 + \frac{1}{1+1}$, *i. e.* $\frac{3}{2}$ is a good deal nearer to $\frac{\pi}{2}$ than $1 + \frac{1}{1+2}$, *i. e.* $\frac{4}{3}$, is; and so $1 + \frac{1}{1+} \frac{1}{\frac{1}{2}+1}$, or $\frac{8}{5}$, is much nearer to it than $1 + \frac{1}{1+} \frac{1}{\frac{1}{2}+3}$, *i. e.* $\frac{16}{9}$, is.

By taking the mean between two consecutive corrected convergents, or, still better, the mean between two such consecutive means, and so on, a few terms will serve to give a very close approximation indeed to the limit $\frac{\pi}{2}$.

† Communicated by the Author.

Suppose any number of equations (to fix the ideas say four) of the form which follows:

$$U = ax + by + cz + dt = 0,$$
$$V = ax^3 + by^3 + cz^3 + dt^3 = 0,$$
$$W = ax^5 + by^5 + cz^5 + dt^5 = 0,$$
$$\Omega = ax^7 + by^7 + cz^7 + dt^7 = 0.$$

If these be regarded as surfaces, they can only be made to intersect in one or another of a definite number of points.

For in the case of intersection we must evidently have

$$dt \cdot \begin{vmatrix} 1 & 1 & 1 & 1 \\ x^2 & y^2 & z^2 & t^2 \\ x^4 & y^4 & z^4 & t^4 \\ x^6 & y^6 & z^6 & t^6 \end{vmatrix} \quad \text{i. e. } dt\zeta(x^2, y^2, z^2, t^2) = 0,$$

ζ being the symbol which expresses the product of the differences of the quantities which it affects. Hence

$$x \pm y = 0 \text{ or } x \pm z = 0 \text{ or } y \pm z = 0 \text{ or } x \pm t = 0 \text{ or } y \pm t = 0$$
$$\text{or } z \pm t = 0 \text{ or } t = 0.$$

Hence it will easily be seen by substitution and successive reduction that the points of intersection are confined to those hereinunder stated and their analogues, viz.

$$x = \pm y = \pm z = \pm t,$$
$$x = \pm y = \pm z, \quad t = 0,$$
$$x = \pm y, \quad z = 0, \quad t = 0,$$
$$x = 0, \quad y = 0, \quad z = 0,$$

the total number of points in the group being

$$2^3 + 4 \cdot 2^2 + 6 \cdot 2 + 4, \text{ i. e. } \frac{3^4 - 1}{2};$$

and so in general for n such equations the number of possible points of intersection will be $\dfrac{3^n - 1}{2}$.

As regards the resultant, we have

$$\begin{vmatrix} 1 & 1 & 1 \\ x^2 & y^2 & z^2 \\ x^4 & y^4 & z^4 \end{vmatrix} \Omega + (\cdot)W + (\cdot)V + (\cdot)U = dt\zeta(x^2, y^2, z^2, t^2).$$

Hence the resultant of U, V, W, Ω is the same as that of

$$U, V, W, dt \cdot \zeta(x^2, y^2, z^2, t^2),$$

divided by the resultant of

$$U, V, W, \zeta(x^2, y^2, z^2),$$

i. e. is the resultant of

$$\text{U, V, W,}\ dt\ (x^2-t^2)(y^2-t^2)(z^9-t^2).$$

This enables us to see that the required resultant is the product of all the resultants of the systems that can be formed by the interchange of a, b, c after the pattern of the system

$$(a\pm d)x\ +(b\pm d)y\ +(c\pm a)z,$$
$$(a\pm d)x^3+(b\pm d)y^3+(c\pm d)z^3,$$
$$(a\pm d)x^5+(b\pm d)y^5+(c\pm d)z^5$$

(the signs in the coefficients of the same column being alike, but independent as between column and column), multiplied by the resultant of

$$ax\ +by\ +cz,$$
$$ax^3+by^3+cz^3,$$
$$ax^5+by^5+cz^5,$$

multiplied by

$$d^{1\,.\,3\,.\,5};$$

and by continuing this process it is obvious that the required resultant will be made up exclusively of factors of the form

$$d^\lambda,\quad (d\pm c)^\mu,\quad (d\pm c\pm b)^\nu,\quad (d\pm c\pm b\pm a)^\pi.$$

So in general for n equations, it may be shown in like manner that the resultant is the product of factors of the form

$$(a_1\pm a_2\pm a_3\ldots\pm a_i)^{u_{n,\,i}},$$

where $u_{n,\,i}$ is a function of n and i to be determined. But by aid of the method of reduction above indicated, and fixing his attention on those factors of the resultant only in which the single coefficient retained in the substituted equation appears, the intelligent reader will find no difficulty in ascertaining

    (1)   that $u_{n,\,1}=1\,.\,3\,.\,5\ldots(2n-1),$

    (2)   that $u_{n,\,i}=(i-1)u_{n-1,\,i-1}.$

These two conditions furnish us with the following Table of double entry :—

$i=$	1,	2,	3,	4,	5,	6
$n=1$	1					
$=2$	1	1				
$=3$	3	1	2			
$=4$	15	3	2	6		
$=5$	105	15	6	6	24	
$=6$	945	105	30	18	24	120

which, of course, may be indefinitely extended.   Thus, *ex. gr.*, when $n=3$, the resultant is

$$(abc)^3(a^2-b^2)(a^2-c^2)(b^2-c^2)(a^4+b^4+c^4-2a^2b^2-2a^2c^2-2b^2c^2)^2.$$

The above investigation leads as a corollary to the following arithmetical theorem.

Call $1.3.5\ldots(2x-1)=Q_x$ and $1=Q_0$.   Then

$$2x\frac{Q_{x-1}}{2}+2x(2x-2)\frac{Q_{x-2}}{4}+2x(2x-2)(2x-4)\frac{Q_{x-4}}{6}+\ldots$$

$$+2x(2x-2)\ldots2.\frac{Q_0}{2x}=\left(\frac{1}{1}+\frac{1}{3}+\frac{1}{5}\ldots+\frac{1}{2x-1}\right)Q_x.$$

Ex. gr. If $x=4$,

$$8\frac{1.3.5}{2}+8.6.\frac{1.3}{4}+8.6.4.\frac{1}{6}+8.6.4.2.\frac{1}{8}$$

$$=60+36+32+48=176.$$

So, too,

$$3.5.7+1.5.7+1.3.7+1.3.5=176.$$

The value of $u_{n,\,i}$ is, of course, $\Pi(i-1)Q_{n-i}$.

There is a more elaborate system of $2n$ equations, the resultant of which can be made to depend on that of the system of n equations just ascertained.   Thus, take $2n=6$, and consider the system

$$ax\ +by\ +cz\ +dt\ +eu\ +fv\,;\quad x\ +y\ +z\ +t\ +u\ +v\,;$$

$$ax^3+by^3+cz^3+dt^3+eu^3+fv^3\,;\quad x^3+y^3+z^3+t^3+u^3+v^3\,;$$

$$ax^5+by^3+cz^5+dt^5+eu^5+fv^3;\quad x^5+y^5+z^5+t^5+u^5+v^5\,;$$

the order of the resultant of this system in the letters a,b,c,d,e,f is obviously $1.3.5(1.3+1.5+3.5)$.

Now *pair* the six variables in every possible manner ; the number of such pairs is $1.3.5$.

Let $x,\,y,\,z,\,t,\,u,\,v$ be any one such set of pairs.   Make

$$x+y=0,\quad z+t=0,\quad u+v=0\,;$$

then the latter set of three functions become zero, and the former three may be made zero with right assignments of $x,\,z,\,t$, provided the resultant of

$$(a-b)x\ +(c-d)z\ +(e-f)u,$$

$$(a-b)x^3+(c-d)z^3+(e-f)u^3,$$

$$(a-b)x^5+(c-d)z^5+(e-f)u^5$$

is zero. Hence the required resultant will *contain* the product of the resultants of the $1.3.5$ systems formed after the above pattern; and as this product will be of $1.3.5(1.3+1.5+3.5)$ dimensions in the constants, it must be not merely contained in, but identical with, the required resultant. Thus the new set of functions regarded as hyper-loci (like the former set) can only be made to intersect in one or another of a fixed group of points. Moreover, passing to the case of $2n$ equations, it is obvious that the resultant of such system will be made up exclusively of factors of the form

$$(a_1 + a_2 + \ldots + a_i - a_{i+1} - a_{i+2} \ldots - a_{2i})^{J_{n,i}} *,$$

where $J_{n,i}$ is a function of n and i to be determined. The value of $u_{n,i}$, which has been found above, leads to this without difficulty. By an obvious method of calculation it may be shown that

$$J_{n,i} = u_{n,i} \left(1.3.5 \ldots (2n-1) \right) \frac{n.(n-1) \ldots (n-i+1)}{1.2 \quad \ldots \quad i} 2^{i-1}$$

$$\div \left\{ \frac{2n.(2n-1) \ldots (2n-2i+1)}{1.2 \quad \ldots \quad 2i} \cdot \frac{1}{2} \cdot \frac{2i.(2i-1) \ldots (i+1)}{1.2 \quad \ldots \quad i} \right\}$$

$$= 2i.2^{i-1} \cdot \frac{\Pi n}{\Pi 2n} \cdot \frac{\Pi(2n-2i)}{\Pi(n-i)} \left(\Pi(i-1) \right)^2 Q_{n-i} \cdot Q_n$$

$$= \Pi(i-1) \Pi i (Q_{n-i})^2 = i(u_{n,i})^2.$$

We thus obtain the following Table for finding the *frequency* $J_{n,i}$ of any given form of factor:—

$i=$	1,	2,	3,	4,	5
$n=1$	1				
$=2$	1	2			
$=3$	9	2	12		
$=4$	225	18	12	144	
$=5$	$(105)^2$	450	108	140	2880

The resultant thus determined is the coefficient of the leading term of an equation of the degree $1^2.3^2.5^2. \ldots .(2n-1)^2$, upon which depends the determination of a set of $2n$ quantities ξ_1, $\xi_2 \ldots, \xi_n$, so chosen as to make the arc of the curve whose equation is

$$y = a_1 \log (x^2 - \xi_1^2) + a_2 \log (x^2 - \xi_2^2) \ldots + a_{2n} \log (x^2 - \xi_{2n}^2)$$

equal to

$$x + a_1 \log \frac{x - \xi_1}{x + \xi_1} + \ldots + a_{2n} \log \frac{x - \xi_{2n}}{x + \xi_{2n}},$$

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* It will, of course, be understood that a_1, a_2, a_3, &c. are written above in place of a, b, c, &c.

$a_1, a_2, \ldots a_{2n}$ being $2n$ given unequal quantities. It follows from the above that the number of distinct solutions is $1^2 . 3^2 \ldots (2n-1)^2$, unless one group of i of the coefficients a and a second group of i other of them can be found such that the sum of the one group is equal to the sum of the other; in that case, and in that case only, the number of solutions undergoes a reduction. A similar conclusion can be extended to the case of an odd number $(2n+1)$ of the parameters (a), in which case the number of solutions is $1^2 . 3^2 \ldots (2n-1)^2(2n+1)$, except when, as above, two sets of parameters can be found the same in number and equal in amount, in which case the number of solutions undergoes a reduction as before.

I mention these facts with the view of making it understood that the problems of elimination herein proposed and solved are not mere idle dreams and speculations of the fancy, but have a real ontological significance in connexion with a great algebraico-Diophantine problem of the Integral Calculus.

P.S. Suppose ν to be any positive integer, even or odd, and that the curve or compound symmetrical logarithmic wave

$$y = \Sigma_{\theta=\nu}^{\theta=1} a_\theta \log (x^2 - \xi_\theta^2)$$

is to be made subject to the relation *arc minus abscissa*

$$= \Sigma_{\theta=\nu}^{\theta=1} a_\theta \log \left(\frac{x-\xi_\theta}{x+\xi_\theta}\right).$$

Then the a coefficients (or form-parameters) being given, the ξ quantities (or asymptotic distances from the Y axis of the logarithmic wavelets) depend on the solution of an algebraical equation whose degree is the product of ν terms of the series

$$1, \ 1, \ 3, \ 3, \ 5, \ 5, \ 7, \ldots$$

When $\nu = 2n$, the coefficient of the leading term of this equation is the resultant of the system, or rather double system, of $2n$ functions of $2n$ variables which has been already discussed.

When $\nu = 2n+1$, the coefficient of the leading term is the resultant of a system of $2n+1$ functions of $2n+1$ variables: $(n+1)$ of them of the form $\Sigma x, \ \Sigma x^3, \ldots \Sigma x^{2n+1}$; n of them of the form $\Sigma ax, \ \Sigma ax^3, \ldots \Sigma ax^{2n+1}$ respectively.

To obtain this last-named resultant we may pair the variables (leaving *one* out) in every possible way, then make the sum of each pair and also the solitary or unpaired one zero, and finally, substituting in the n equations last stated (which come down to the form of a system of n equations between n variables discussed at the outset of this paper), calculate its resultant*. The product

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* Regarded as loci, the ν functions can only interset in one or another of an invariable system of points independent of the particular values of the coefficients. The equations to any one of these points (from what has

of all the resultants so found will be the resultant required, as may be proved by counting its order in the given coefficients, which is easily ascertained to be

$$1 \cdot 3 \cdot 5 \dots \overline{2n+1} \left(\frac{1}{1} + \frac{1}{3} + \dots \frac{1}{2n-1} \right) (1 \cdot 3 \cdot 5 \dots \overline{2n-1}),$$

as it ought in order to be the complete resultant. It will be seen then that this complete resultant, like the former one, is still made up of linear factors of the form

$$(a_1 + a_2 + \dots + a_i - a_{i+1} - a_{i+2} \dots - a_{2i}),$$

and it only remains to ascertain the *frequency* of each such factor. By a calculation precisely similar in nature to that indicated for the case of $\nu = 2n$, it will be found that for this case of $\nu = 2n+1$ the *frequency* in question

$$= \Pi(i-1)\Pi i \cdot Q(n-i)Q(n-i+1).$$

For $\nu = 2n$ it has been already proved to be

$$\Pi(i-1)\Pi i \big(Q(n-i) \big)^2.$$

Thus we obtain the complete double-entry Table of *Frequency* underwritten :

$i=$	1,	2,	3,	4,	5,	6
$\nu = 2$	1					
$= 3$	1					
$= 4$	1	2				
$= 5$	3	2				
$= 6$	9	2	12			
$= 7$	45	6	12			
$= 8$	225	18	12	144		
$= 9$	1575	90	36	144		
$=10$	11025	450	108	144	2880	
$=11$	99225	3150	540	432	2880	
$=12$	893025	22050	2700	1296	2880	3628800

This Table, although obtained by two slightly varying pro-

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been shown in the text) will easily be seen to be of the form

$$\left\{ \begin{array}{l} x_1 = x_2 = \dots = x_{2i} = -x_{2i+1} = -x_{2i+2} = \dots = -x_{2j}, \\ x_{2j+1} = 0, \quad x_{2j+2} = 0, \dots x_\gamma = 0. \end{array} \right\}$$

Hence by a simple enough combinatorial calculation it may be deduced that the number of these fixed possible points of intersection, or, so to say, ganglions of the system is $\dfrac{3^\gamma + (-1)^\gamma}{8} - \dfrac{1}{4}$, which is, of course, always an integer; or, more briefly, the ganglionic exponent is the integer part of $\dfrac{3^\gamma}{8}$.

cesses according as ν is even or odd, forms, and ought to be re-garded as, an organic whole.

To prevent misconception, I ought to add that when ν is suf-ficiently great, the compound symmetrical logarithmic wave $\Sigma a \log (x^2 - \xi^2)$ admits of other rectifiable cases besides those of the form $\Sigma a \log \left(\dfrac{x - \xi}{x + \xi}\right)$ above adduced.

It remains to study the relation between the frequency of each factor and the nature of the corresponding contact between the functions (regarded as *loci*) into whose resultant it enters. I have reason to hope that Dr. Olaus Henrici, who has done such valuable work in the theory of Discriminants and Resultants, may be disposed to take up this interesting and pregnant question.

LV. *Notices respecting New Books.*

Physical and Historical Evidences of vast Sinkings of Land on the North and West Coasts of France, and South-western Coasts of England, within the Historical Period. Collected and com-mented on by R. A. Peacock, *Esq.,* C.E. 8vo. London and St. Helier, 1868.

IN studying the Geology of Jersey, the author was struck by the strong evidences of subsidence in the sea about that island and the neighbouring coasts ; and turning his attention to the subject, and being already strongly imbued with notions respecting the existence of cavities in the earth admitting of, and conducive to, infallings, he took up the matter with energy, and, gathering information from the antiquary, astronomer, engineer, naturalist, geologist, geogra-pher, historian, and philologist, he has compiled and digested the great number of facts and statements carefully arranged in this closely printed and very interesting book, which is illustrated by three outline maps. These collected evidences prove that within the last nineteen centuries, and even so late as the beginning of the fifteenth century, large tracts of land and sea-bottom have sunk, even more than a hundred feet at some places, along the coasts of Western Prussia, Holland, and Belgium, from the Elbe to near Nieuport ; along the coasts of North Somerset, and of Devon and Cornwall, north and south ; in the bed of the English Channel ; amongst the Channel Islands ; along the coast of Normandy and Brittany, from the Seine to Portrieux ; on the north coast of Brit-tany, from about Lannion to the north-west angle of Brittany ; around the Isle of Sein, on the west of Brittany ; and probably also along the French coast in the Bay of Biscay ; whilst possibly the land around Rochelle has risen a few feet since the commencement of the twelfth century. The rich store here accumulated of evi-dences of loss of land, particularly among the Channel Islands and on the north and west coasts of France, has been carefully gleaned

by observation, correspondence, and reading, from all available sources, especially from geography and history, ancient, mediæval, and modern.

Nowadays most educated people can understand to some extent, and are prepared to believe in, oscillations of the land, and can even master the idea of the English Channel and the German Ocean having enlarged their areas by subsidence within some appreciable period of time ; such a book as the present gives them proofs enough of this having occurred, and supplies much other matter for useful thought. The geologist, of course, can accept the facts and conclusions readily ; for he knows that these changes which have happened in the relative level of land and water during historic times are but modern repetitions of movements that have long affected this portion of the earth's crust. Some of the results of these normal disturbances of level, in the prehistoric and pliocene periods, have been described with scientific accuracy by Mr. R. Godwin-Austen (in the Journal of the Geological Society of London, in 1849–51), well known among geologists for his mastery of the geography of the past, and as a delineator of the old hydrographical conditions of the earth's surface, altered again and again by heavings and sinkings of the land. Thanking Mr. Peacock most heartily, therefore, for the valuable repertory of facts his book presents, we need not be at all astonished at the changes that have occurred in North-western Europe in the last few centuries, whether ten, twenty, a hundred, or more ; for already we have learnt from Godwin-Austen's good geological observation and reasoning " that the area of the present English Channel was in the condition of dry land previous to its occupation by the waters of the Pleistocene sea, or during the period of the Pliocene (Crag) accumulations of the German basin, and that, together with a large area beyond, it served to connect the British Islands with France on the south, and Ireland on the west, into a tract which had a far greater amount of elevation than any portion of it has at present." Books and parchments are wanting for these long-past pre-historic times, but kitchen-middens and flint implements, and even human bones, have already supplied some indications of early man and his occupation of Europe and Britain when at an elevation greatly differing from what now exists ; and much more is still to be learnt of this highly interesting subject by careful work in the line of research that has been followed by Mr. Peacock among engineers and by Mr. Godwin-Austen among geologists.

On Steam as the Motive Power in Earthquakes and Volcanoes, and on Cavities in the Earth's Crust. By R. A. PEACOCK, *Esq., C.E.* 8vo. Jersey, 1866.

Bound up with the foregoing, and accompanied with a Preface dated 1867, is a tract with the title given above. In it the well-known fact that the vapour of water is the cause of volcanic explosions and other phenomena is enthusiastically insisted upon. The many quotations from observers and writers brought forward in illustration of this long-accepted theory would delight Woodward,

Mitchell, and the other old vaporists, were they to rise now amongst their successors and take part in discussions on the earthquake question.

The author offers a new formula for calculating the temperature of high-pressure steam, and some carefully worked-out tables of temperatures and pressures of high-pressure steam, and says (in his Prospectus), "it has been ascertained that, within known limits at least, the force of saturated steam increases as the $4\frac{1}{2}$-power of the temperature, this being an enormously rapid ratio of increase. For example, the $4\frac{1}{2}$-power of the number 10 is equal to 31,622 and a fraction. In this way we may understand how melted lava, which has a temperature of about 3000° Fahr., may, when water gains access to it, produce steam sufficiently powerful to account for the greatest effects of earthquakes and volcanoes." Mr. Peacock's studies on the properties of steam are useful adjuncts to the extensive researches of Davy, Daubeny, Scrope, Daubrée, and others on the part played by water in the reaction of the interior of the earth against its crust ; but when, led into those subterranean regions of hypothesis where cavities or no cavities, solid nucleus or no nucleus, hot centre or cool centre, are the vexed and vexing questions of the scientific physicist, mathematical amateur, and general newspaper-reader, he gives the usual compilation of facts and fancies, we can only hope that some at least of his observations will be of use to somebody— that he will enlarge his reading in the modern literature of the subject—not mix up the little superficial caverns of limestone, lava, and such like with the larger cavities in the earth's crust, for the existence of which he judiciously argues—and, lastly, that he will take into consideration the conditions and effects of Dana's theory of the horizontal crumpling of mountain-masses and other strata by lateral crush or contraction, before he allows everything to be absorbed by hypothetic cavities.

LVI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 308.]

Jan. 14, 1869.—Lieut.-General Sabine, President, in the Chair.

THE following communication was read :—
  "On the Blue Colour of the Sky, the Polarization of Skylight, and on the Polarization of Light by Cloudy matter generally." By John Tyndall, LL.D., F.R.S.

Since the communication of my brief abstract "On a new Series of Chemical Reactions produced by Light," the experiments upon this subject have been continued, and the number of the substances thus acted on considerably augmented. New relations have also been established between *mixed vapours* when subjected to the action of light.

I now beg to draw the attention of the Royal Society to two

questions glanced at incidentally in the abstract referred to—the blue colour of the sky, and the polarization of skylight. Reserving the historic treatment of the subject for a more fitting occasion, I would merely mention now that these questions constitute, in the opinion of our most eminent authorities, the two great standing enigmas of meteorology. Indeed it was the interest manifested in them by Sir John Herschel, in a letter of singular speculative power, that caused me to enter upon the consideration of these questions so soon.

The apparatus with which I work consists, as already stated to the Society, of a glass tube about a yard in length, and from $2\frac{1}{2}$ to 3 inches in internal diameter. The vapour to be examined is introduced into this tube in the manner described in my last abstract, and upon it the condensed beam of the electric lamp is permitted to act until the neutrality or the activity of the substance has been declared.

It has hitherto been my aim to render the chemical action of light upon vapours *visible*. For this purpose substances have been chosen, *one* at least of whose products of decomposition under light shall have a boiling-point so high that as soon as the substance is formed it shall be *precipitated*. By graduating the quantity of the vapour, this precipitation may be rendered of any degree of fineness, forming particles distinguishable by the naked eye, or particles which are probably far beyond the reach of our highest microscopic powers.

I have no reason to doubt that particles may be thus obtained whose diameters constitute but a very small fraction of the length of a wave of violet light.

In all cases when the vapours of the liquids employed are suffi- ciently attenuated, no matter what the liquid may be, the visible action commences with the formation of a *blue cloud*. I would guard myself at the outset against all misconception as to the use of this term. The blue cloud to which I here refer is totally invisible in ordinary daylight. To be seen, it requires to be surrounded by dark- ness, *it only* being illuminated by a powerful beam of light. This blue cloud differs in many important particulars from the finest ordi- nary clouds, and might justly have assigned to it an intermediate po- sition between these clouds and true cloudless vapour.

With this explanation, the term "cloud," or "incipient cloud," as I propose to employ it, cannot, I think, be misunderstood.

I had been endeavouring to decompose carbonic acid gas by light. A faint bluish cloud, due it may be, or it may not be, to the residue of some vapour previously employed, was formed in the experi- mental tube. On looking across this cloud through a Nicol's prism, the line of vision being horizontal, it was found that when the short diagonal of the prism was vertical the quantity of light reaching the eye was greater than when the long diagonal was vertical.

When a plate of tourmaline was held between the eye and the bluish cloud, the quantity of light reaching the eye when the axis of the prism was perpendicular to the axis of the illuminating beam was greater than when the axes of the crystal and of the beam were parallel to each other.

This was the result all round the experimental tube. Causing the crystal of tourmaline to revolve round the tube, with its axis perpen-

dicular to the illuminating beam, the quantity of light that reached the eye was in all its positions a maximum ; when the crystallographic axis was parallel to the axis of the beam, the quantity of light transmitted by the crystal was a minimum.

From the illuminated bluish cloud, therefore, polarized light was discharged, the direction of maximum polarization being at right angles to the illuminating beam ; the *plane of vibration* of the polarized light, moreover, was that to which the beam was perpendicular*.

Thin plates of selenite or of quartz, placed between the Nicol and the bluish cloud, displayed the colours of polarized light, these colours being most vivid when the line of vision was at right angles to the experimental tube. The plate of selenite usually employed was a circle, thinnest at the centre, and augmenting uniformly in thickness from the centre outwards. When placed in its proper position between the Nicol and the cloud, it exhibited a system of splendidly coloured rings.

The cloud here referred to was the first operated upon in the manner described. It may, however, be greatly improved upon by the choice of proper substances, and by the application in proper quantities of the substances chosen. Benzol, bisulphide of carbon, nitrite of amyl, nitrite of butyl, iodide of allyl, iodide of isopropyl, and many other substances may be employed. I will take the nitrite of butyl as illustrative of the means adopted to secure the best result with reference to the present question.

And here it may be mentioned that a vapour, which when alone, or mixed with air in the experimental tube, resists the action of light, or shows but a feeble result of this action, may, by placing it in proximity with another gas or vapour, be caused to exhibit under light vigorous, if not violent action. The case is similar to that of carbonic acid gas, which diffused in the atmosphere resists the decomposing action of solar light, but when placed in contiguity with the chlorophyl in the leaves of plants has its molecules shaken asunder.

Dry air was permitted to bubble through the liquid nitrite of butyl until the experimental tube, which had been previously exhausted, was filled with the mixed air and vapour. The visible action of light upon the mixture after fifteen minutes' exposure was slight. The tube was afterwards filled with half an atmosphere of the mixed air and vapour, and another half atmosphere of air which had been permitted to bubble through fresh commercial hydrochloric acid. On sending the beam through this mixture, the action paused barely sufficiently long to show that at the moment of commencement the tube was optically empty. But the pause amounted only to a small fraction of a second, a dense cloud being immediately precipitated upon the beam which traversed the mixture.

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* I assume here that the plane of vibration is perpendicular to the plane of polarization. This is still an undecided point ; but the probabilities are so much in its favour, and it is in my opinion so much preferable to have a physical image on which the mind can rest, that I do not hesitate to employ the phraseology in the text. Even should the assumption prove to be incorrect, no harm will be done by the provisional use of it.

This cloud began *blue*, but the advance to whiteness was so rapid as almost to justify the application of the term instantaneous. The dense cloud, looked at perpendicularly to its axis, showed scarcely any signs of polarization ; looked at obliquely the polarization was strong.

The experimental tube being again cleansed and exhausted, the mixed air and nitrite-of-butyl vapour was permitted to enter it until the associated mercury column was depressed $\frac{1}{10}$ of an inch. In other words, the air and vapour, united, exercised a pressure not exceeding $\frac{1}{300}$ of an atmosphere. Air passed through a solution of hydrochloric acid was then added till the mercury column was depressed three inches. The condensed beam of the electric light passed for some time in darkness through this mixture. There was absolutely nothing within the tube competent to scatter the light. Soon, however, a superbly blue cloud was formed along the track of the beam, and it continued blue sufficiently long to permit of its thorough examination. The light discharged from the cloud at right angles to its own length was *perfectly* polarized. By degrees the cloud became of a whitish blue, and for a time the selenite colours obtained by looking at it normally were exceedingly brilliant. The direction of maximum polarization was distinctly at right angles to the illuminating beam. This continued to be the case as long as the cloud maintained a decided blue colour, and even for some time after the pure blue had changed to whitish blue. But as the light continued to act the cloud became coarser and whiter, particularly at its centre, where it at length ceased to discharge polarized light in the direction of the perpendicular, while it continued to do so at both its ends.

But the cloud which had thus ceased to polarize the light emitted normally, showed vivid selenite colours when looked at *obliquely*. The direction of maximum polarization changed with the texture of the cloud. This point shall receive further illustration subsequently.

A blue, equally rich and more durable, was obtained by employing the nitrite-of-butyl vapour in a still more attenuated condition. Now the instance here cited is *representative*. In all cases, and with all substances, the cloud formed at the commencement, when the precipitated particles are sufficiently fine, is *blue*, and it can be made to display a colour rivalling that of the purest Italian sky. In all cases, moreover, this fine blue cloud polarizes *perfectly* the beam which illuminates it, the direction of polarization enclosing an angle of 90° with the axis of the illuminating beam.

It is exceedingly interesting to observe both the perfection and the decay of this polarization. For ten or fifteen minutes after its first appearance the light from a vividly illuminated incipient cloud, looked at horizontally, is absolutely quenched by a Nicol's prism with its longer diagonal vertical. But as the sky-blue is gradually rendered impure by the introduction of particles of too large a size—in other words, as real clouds begin to be formed, the polarization begins to deteriorate, a portion of the light passing through the prism in all its positions. It is worthy of note that, for some time after the cessation of perfect polarization, the *residual* light which passes when the Nicol is in its position of minimum transmission is of a gorgeous

blue, the whiter light of the cloud being extinguished*. When the cloud-texture has become sufficiently coarse to approximate to that of ordinary clouds, the rotation of the Nicol ceases to have any sensible effect on the quality of the light discharged normally.

The perfection of the polarization in a direction perpendicular to the illuminating beam is also illustrated by the following experiment. A Nicol's prism large enough to embrace the entire beam of the electric lamp was placed between the lamp and the experimental tube. A few bubbles of air carried through the liquid nitrite of butyl were introduced into the tube, and they were followed by about 3 inches (measured by the mercurial gauge) of air which had been passed through aqueous hydrochloric acid. Sending the polarized beam through the tube, I placed myself in front of it, my eye being on a level with its axis, my assistant, Mr. Cottrell, occupying a similar position behind the tube. The short diagonal of the large Nicol was in the first instance vertical, the plane of vibration of the emergent beam being therefore also vertical. As the light continued to act, a superb blue cloud visible to both my assistant and myself was slowly formed. But this cloud, so deep and rich when looked at from the positions mentioned, *utterly disappeared when looked at vertically downwards, or vertically upwards.* Reflection from the cloud was not possible in these directions. When the large Nicol was slowly turned round its axis, the eye of the observer being on the level of the beam, and the line of vision perpendicular to it, entire extinction of the light emitted horizontally occurred when the longer diagonal of the large Nicol was vertical. But now a vivid blue cloud was seen when looked at downwards or upwards. This truly fine experiment was first definitely suggested by a remark addressed to me in a letter by Prof. Stokes.

Now, as regards the polarization of skylight, the greatest stumblingblock has hitherto been that, in accordance with the law of Brewster (which makes the index of refraction the tangent of the polarizing-angle), the reflection which produces perfect polarization would require to be made *in* air *upon* air ; and indeed this led many of our most eminent men, Brewster himself among the number, to entertain the idea of *molecular reflection.* I have, however, operated upon substances of widely different refractive indices, and therefore of very different polarizing-angles as ordinarily defined, but the polarization of the beam by the incipient cloud has thus far proved itself to be *absolutely independent of the polarizing-angle.* The law of Brewster does not apply to matter in this condition ; and it rests with the undulatory theory to explain why. Whenever the precipitated particles are sufficiently fine, no matter what the substance forming the particles may be, the direction of maximum polarization is at right angles to the illuminating beam, the polarizing angle for matter in this condition being invariably 45°. This I consider to be a point of capital importance with reference to the present question†.

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* This seems to prove that particles too large to polarize the blue, polarize perfectly light of lower refrangibility.

† The difficulty referred to above is thus expressed by Sir John Herschel :—
" The cause of the polarization is evidently a reflection of the sun's light upon *something.* The question is, On what ? Were the angle of maximum polarization

That *water-particles,* if they could be obtained in this exceedingly fine state of division, would produce the same effects, does not admit of reasonable doubt. And that they must exist in this condition in the higher regions of the atmosphere is, I think, certain. At all events, no other assumption than this is necessary to completely account for the firmamental blue and the polarization of the sky*.

Suppose our atmosphere surrounded by an envelope impervious to light, but with an aperture on the sunward side through which a parallel beam of solar light could enter and traverse the atmosphere. Surrounded on all sides by air not directly illuminated, the track of such a beam through the air would resemble that of the parallel beam of the electric lamp through an incipient cloud. The sunbeam would be *blue,* and it would discharge laterally light in precisely the same condition as that discharged by the incipient cloud. In fact the azure revealed by such a beam would be to all intents and purposes that which I have called a "blue cloud" †.

But, as regards the polarization of the sky, we know that not only is the direction of maximum polarization at right angles to the track of the solar beams, but that at certain angular distances, probably variable ones, from the sun "neutral points" (or points of no polarization) exist, on both sides of which the planes of atmospheric polarization are at right angles to each other.

I have made various observations upon this subject which I reserve for the present; but, pending the more complete examination of the question, the following facts and observations bearing upon it are submitted to the Royal Society.

The parallel beam employed in these experiments marked its way through the laboratory-air exactly as sun-beams are seen to do in the dusty air of London. I have reason to believe that a great por-

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76°, we should look to water or ice as the reflecting body, however inconceivable the existence in a cloudless atmosphere on a hot summer's day of unevaporated molecules (particles?) of water. But though we were once of this opinion, careful observation has satisfied us that 90°, or thereabouts, is a correct angle, and that therefore, whatever be the body on which the light has been reflected, *if polarized by a single reflection,* the polarizing angle must be 45°, and the index of refraction, which is the tangent of that angle, unity; in other words, the reflection would require to be made *in* air *upon* air!" ('Meteorology,' par. 233).

* Any particles, if small enough, will produce both the colour and the polarization of the sky. But is the existence of small water-particles on a hot summer's day *in the higher regions of our atmosphere* inconceivable? It is to be remembered that the oxygen and nitrogen of the air behave as a vacuum to radiant heat, the exceedingly attenuated vapour of the higher atmosphere being therefore in practical contact with the cold of space.

† The opinion of Sir John Herschel, connecting the polarization and the blue colour of the sky, is verified by the foregoing results. "The more the subject [the polarization of skylight] is considered," writes this eminent philosopher, "the more it will be found beset with difficulties; and its explanation, when arrived at, will probably be found to carry with it that of the blue colour of the sky itself and of the great quantity of light it actually does send down to us." "We may observe, too," he adds, "that it is only where the purity of the sky is most absolute that the polarization is developed in its highest degree, and that where there is the slightest perceptible tendency to cirrus it is materially impaired." This applies, word for word, to the "incipient clouds."

tion of the matter thus floating in the laboratory-air consists of organic germs, which are capable of imparting a perceptibly bluish tint to the air. This air showed, though far less vividly, all the effects of polarization obtained with the incipient clouds. The light discharged laterally from the track of the illuminating beam was polarized, though not perfectly, the direction of maximum polarization being at right angles to the beam.

The horizontal column of air thus illuminated was 18 feet long, and could therefore be looked at very obliquely without any disturbance from a solid envelope. At all points of the beam throughout its entire length the light emitted normally was in the same state of polarization. Keeping the positions of the Nicol and the selenite constant, the same colours were observed throughout the entire beam when the line of vision was perpendicular to its length.

I then placed myself near the end of the beam as it issued from the electric lamp, and, looking through the Nicol and selenite more and more obliquely at the beam, observed the colours fading until they disappeared. Augmenting the obliquity, the colours appeared once more, *but they were now complementary to the former ones.*

Hence this beam, like the sky, exhibited its neutral point, at opposite sides of which the light was polarized in planes at right angles to each other.

Thinking that the action observed in the laboratory might be caused in some way by the vaporous fumes diffused in its air, I had a battery and an electric lamp carried to a room at the top of the Royal Institution. The track of the beam was seen very finely in the air of this room, a length of 14 or 15 feet being attainable. This beam exhibited all the effects observed with the beam in the laboratory. Even the uncondensed electric light falling on the floating matter showed, though faintly, the effects of polarization*.

When the air was so sifted as to entirely remove the visible floating matter, it no longer exerted any sensible action upon the light, but behaved like a vacuum.

I had varied and confirmed in many ways those experiments on neutral points, operating upon the fumes of chloride of ammonium, the smoke of brown paper, and tobacco-smoke, when my attention was drawn by Sir Charles Wheatstone to an important observation communicated to the Paris Academy in 1860 by Professor Govi, of Turin†. His observations on the light of comets had led M. Govi to examine a beam of light sent through a room in which was diffused the smoke of incense. He also operated on tobacco-smoke. His first brief communication stated the fact of polarization by such smoke; but in his second communication he announced the discovery of a neutral point in the beam, at the opposite sides of which the light was polarized in planes at right angles to each other.

But, unlike my observations on the laboratory-air, and unlike the action of the sky, the direction of maximum polarization in M. Govi's experiment enclosed a very small angle with the axis of the illuminating beam. The question was left in this condition, and I

* I hope to try Alpine air next summer.
† Comptes Rendus, tome li. pp. 360 & 669.

am not aware that M. Govi or any other investigator has pursued it further.

I had noticed, as before stated, that as the clouds formed in the experimental tube became denser, the polarization of the light discharged at right angles to the beam became weaker, the direction of maximum polarization becoming oblique to the beam. Experiments on the fumes of chloride of ammonium gave me also reason to suspect that the position of the neutral point *was not constant*, but that it varied with the density of the illuminated fumes.

The examination of these questions led to the following new and remarkable results :—The laboratory being well filled with the fumes of incense, and sufficient time being allowed for their uniform diffusion, the electric beam was sent through the smoke. From the track of the beam polarized light was discharged, but the direction of maximum polarization, instead of being along the normal, now enclosed an angle of 12° or 13° with the axis of the beam.

A neutral point, with complementary effects at opposite sides of it, was also exhibited by the beam. The angle enclosed by the axis of the beam, and a line drawn from the neutral point to the observer's eye, measured in the first instance 66°.

The windows of the laboratory were now opened for some minutes, a portion of the incense smoke being permitted to escape. On again darkening the room and turning on the beam, the line of vision to the neutral point was found to enclose with the axis of the beam an angle of 63°.

The windows were again opened for a few minutes, more of the smoke being permitted to escape. Measured as before, the angle referred to was found to be 54°.

This process was repeated three additional times ; the neutral point was found to recede lower and lower down the beam, the angle between a line drawn from the eye to the neutral point and the axis of the beam falling successively from 54° to 49°, 43°, and 33°.

The distances, roughly measured, of the neutral point from the lamp, corresponding to the foregoing series of observations, were these :—

1st observation,		2 feet		2 inches.	
2nd	,,	2	,,	6	,,
3rd	,,	2	,,	10	,,
4th	,,	3	,,	2	,,
5th	,,	3	,,	7	,,
6th	,,	4	,,	6	,,

At the end of this series of experiments the direction of maximum polarization had again become normal to the beam.

The laboratory was next filled with the fumes of gunpowder. In five successive experiments, corresponding to five different densities of the gunpowder-smoke, the angles enclosed between the line of vision to the neutral point and the axis of the beam were 63°, 50°, 47°, 42°, and 38° respectively.

After the clouds of gunpowder had cleared away, the laboratory was filled with the fumes of common resin, rendered so dense as to be

very irritating to my lungs.   The direction of maximum polarization enclosed in this case an angle of 12°, or thereabouts, with the axis of the beam.   Looked at, as in the former instances, from a position near the electric lamp, *no neutral point* was observed throughout the entire extent of the beam.

When this beam was looked at normally through the selenite and Nicol, the ring system, though not brilliant, was distinct.   Keeping the eye upon the plate of selenite and the line of vision normal, the windows were opened, the blinds remaining undrawn.   The resinous fumes slowly diminished, and as they did so the ring system became paler ; it finally disappeared.   Continuing to look along the perpendicular, the rings revived, but now the colours were complementary to the former ones.   *The neutral point had passed me in its motion down the beam consequent upon the attenuation of the fumes of resin.*

With the fumes of chloride of ammonium substantially the same results were obtained as those just described.   Sufficient, I think, has been here stated to illustrate the variability of the position of the neutral point.   The explanation of the results will probably give new work to the undulatory theory*.

Before quitting the question of the reversal of the polarization by cloudy matter, I will make one or two additional observations. Some of the clouds formed in the experiments on the chemical action of light are astonishing as to form.   The experimental tube is often divided into segments of dense cloud, separated from each other by nodes of finer matter.   Looked at normally, as many as four reversals of the plane of polarization have been found in the tube in passing from node to segment, and from segment to node.   With the fumes diffused in the laboratory, on the contrary, there was no change in the polarization along the normal ; for here the necessary differences of cloud-texture did not exist.

Further, by a puff of tobacco-smoke or of condensed steam blown into the illuminated beam, the brilliancy of the colours may be greatly augmented.   But with different clouds two different effects are produced.   For example, let the ring system observed in the common air be brought to its maximum strength, and then let an attenuated cloud of chloride of ammonium be thrown into the beam at the point looked at ; the ring system flashes out with augmented brilliancy, and the character of the polarization remains unchanged. This is also the case when phosphorus or sulphur is burned underneath the beam, so as to cause the fine particles of phosphoric acid or of sulphur to rise into the light.   With the sulphur-fumes the brilliancy of the colours is exceedingly intensified ; but in none of these cases is there any change in the character of the polarization.

But when a puff of aqueous cloud, or of the fumes of hydrochloric acid, hydriodic acid, or nitric acid is thrown into the beam, there is a complete reversal of the selenite tints.   Each of these clouds

* Brewster has proved the variability of the position of the neutral point for skylight with the sun's altitude.   Is not the proximate cause of this revealed by the foregoing experiments?

twists the plane of polarization 90°. On these and kindred points experiments are still in progress*.

The idea that the colour of the sky is due to the action of finely divided matter, rendering the atmosphere a turbid medium through which we look at the darkness of space, dates as far back as Leonardo da Vinci. Newton conceived the colour to be due to exceedingly small water particles acting as thin plates. Goethe's experiments in connexion with this subject are well known and exceedingly instructive. One very striking observation of Goethe's referred to what is technically called "chill" by painters, which is due no doubt to extremely fine varnish particles interposed between the eye and a dark background. Clausius, in two very able memoirs, endeavoured to connect the colours of the sky with suspended water vesicles, and to show that the important observations of Forbes on condensing steam could also be thus accounted for. Brücke's experiments on precipitated mastic were referred to in my last abstract. Helmholtz has ascribed the blueness of the eyes to the action of suspended particles. In an article written nearly nine years ago by myself, the colours of the peat-smoke of the cabins of Killarney† and the colours of the sky were referred to one and the same cause, while a chapter of the 'Glaciers of the Alps,' published in 1860, is also devoted to this question. Roscoe, in connexion with his truly beautiful experiments on the photographic power of sky-light, has also given various instances of the production of colour by suspended particles. In the foregoing experiments the azure was produced in *air*, and exhibited a depth and purity far surpassing anything that I have ever seen in mote-filled liquids. Its polarization, moreover, was *perfect*.

In his experiments on fluorescence Professor Stokes had continually to separate the light reflected from the motes suspended in his liquids (the action of which he named "false dispersion") from the fluorescent light of the same liquids (which he ascribed to "true dispersion"). In fact it is hardly possible to obtain a liquid without motes, which polarize by reflection the light falling upon them, truly dispersed light being unpolarized. At p. 530 of his celebrated memoir "On the Change of the Refrangibility of Light," Prof. Stokes adduces some significant facts, and makes some noteworthy remarks, which bear upon our present subject. He notices more particularly a specimen of plate glass which, seen by reflected light, exhibited a blue which was exceedingly like an effect of fluorescence, but which, when properly examined, was found to be an instance of false dispersion. "It often struck me," he writes, "while engaged in these observations, that when the beam had a continuous appearance the polarization was more nearly perfect than when it was sparkling, so

* Sir John Herschel has suggested to me that this change of the polarization from positive to negative may indicate a change from polarization by reflection to polarization by refraction. This thought repeatedly occurred to me while looking at the effects; but it will require much following up before it emerges into clearness.

† I have sometimes quenched almost completely, by a Nicol, the light discharged normally from burning leaves in Hyde Park. The blue smoke from the *ignited end* of a cigar polarizes also, but not perfectly.

as to force on the mind the conviction that it arose merely from motes*. Indeed in the former case the polarization has often appeared perfect, or all but perfect. It is possible that this may in some measure have been due to the circumstance that, when a given quantity of light is diminished in a given ratio, the illumination is perceived with more difficulty when the light is diffused uniformly than when it is spread over the same space but collected into specks. Be this as it may, there was at least no tendency observed towards polarization in a plane perpendicular to the plane of reflection when the suspended particles became finer, and therefore the beam more nearly continuous."

Through the courtesy of its owner, I have been permitted to see and to experiment with the piece of plate glass above referred to. Placed in front of the electric lamp, whether edgeways or transversely, it discharges bluish polarized light laterally, the colour being by no means a bad imitation of the blue of the sky.

Prof. Stokes considers that this deportment may be invoked to decide the question of the direction of the vibrations of polarized light. On this point I would say, if it can be demonstrated that when the particles are small in comparison to the length of a wave of light the vibrations of a ray reflected by such particles cannot be perpendicular to the vibrations of the incident light, then assuredly the experiments recorded in the foregoing communication decide the question in favour of Fresnel's assumption.

As stated above, almost all liquids have motes in them sufficiently numerous to polarize sensibly the light; and very beautiful effects may be obtained by simple artificial devices. When, for example, a cell of distilled water is placed in front of the electric lamp, and a slice of the beam permitted to pass through it, scarcely any polarized light is discharged, and scarcely any colour produced by a plate of selenite. But while the beam is passing through it, if a bit of soap be agitated in the water above the beam, the moment the infinitesimal particles reach the beam the liquid sends forth laterally almost perfectly polarized light; and if the selenite be employed, vivid colours flash into existence. A still more brilliant result is obtained with mastic dissolved in a great excess of alcohol.

The selenite rings constitute an extremely delicate test as to the quantity of motes in a liquid. Commencing with distilled water, for example, a thickish beam of light is necessary to make the polarization of its motes sensible. A much thinner beam suffices for common water; while with Brücke's precipitated mastic, a beam too thin to produce any sensible effect with most other liquids suffices to bring out vividly the selenite colours.

* The azure may be produced in the midst of a field of motes. By turning the Nicol, the interstitial blue may be completely quenched, the shining and apparently unaffected motes remaining masters of the field. A blue cloud, moreover, may be precipitated in the midst of the azure. An aqueous cloud thus precipitated reverses the polarization; but on the melting away of the cloud the azure and its polarization remain behind.

ROYAL INSTITUTION OF GREAT BRITAIN.

March 19, 1869.—" On Chemical Constitution, and its Relation
to Physical and Physiological Properties." By Dr. A. Crum Brown,
F.R.S.E.

Chemists have long endeavoured to answer the question, What
is the relation in which the constituents stand to one another in a
compound ? and numerous hypotheses, more or less ingenious, have
been devised for this purpose. Two of these modes of representing
chemical phenomena occupy so prominent a place in the history of
the science as to merit special notice, even in so slight and hurried
a sketch as this must be. These are, 1st, the Electro-chemical and
Radical Theory ; and 2nd, the Theory of Atomicity and Chemical
Structure.

The first was the product of the genius, learning, and laborious
research of Berzelius ; it was soon adopted by all chemists, and
formed for many years the foundation of all chemical teaching and
the guide in all chemical work. The point of view from which it
regards chemical phenomena is that of combination and decomposi-
tion, of the union of elements to form compounds, and the separa-
tion of compounds into elements. A very important form of
chemical decomposition is electrolysis, or the breaking up of a com-
pound by means of current electricity. From the nature of the case
electrolysis gives rise to a dichotomous decomposition ; and this
duality was extended to all cases of combination and decomposition.
Elements combine with each other in pairs ; these pairs may again
combine in pairs, forming compounds of the second order, and so on.
Thus calcium combines with oxygen to form lime, sulphur combines
with oxygen to form sulphuric acid, and sulphuric acid combines
with lime to form sulphate of lime. This union of compounds with
compounds was not supposed to depend on a union of the con-
stituents of the one with the constituents of the other, but to be a
combination of the one as a whole with the other as a whole—not
a combination of the calcium of the lime with the sulphur or with
the oxygen of the sulphuric acid, or of the sulphur of the sulphuric
acid with the oxygen of the lime, but of the lime as such with the
sulphuric acid as such.

This view may be illustrated by a reference to the relations of
human life. Individuals unite to form partnerships or corporations ;
and these may again enter into alliances, although the members of
the one allied corporation may be altogether unacquainted and
unconnected with the members of the other.

But the progress of discovery brought to light facts which seemed
to contradict this view of binary combination. Cases were observed
in which a compound of two elements united directly with an
element ; and to meet this new class of facts the theory was modified
by the introduction of the notion of Radicals. A radical was a
compound which acts like an element.

The simile introduced above may be used to illustrate this exten-
sion of the theory. Some combinations of men (corporations) can
be treated as individuals, can enter into legal relations with indivi-

duals, while others cannot ; so some compounds can unite with ele-
ments, while others have not this capability.

The Theory of Atomicity regards chemical phenomena from an
altogether different point of view.   In it the various substances are
considered as modifications of one another rather than as compounds.
The rise of this mode of viewing chemical phenomena may be traced
from the early papers by Dumas, and by Laurent, on Substitution.
It appears more prominently in the position given to double de-
composition as the representative of all chemical action, by Laurent
and Gerhardt, in the types of Gerhardt and Williamson, in Frank-
land's theory of the organo-metallic bodies, and in its extension by
Kolbe to the compounds of carbon.   It was reserved, however, for
Kekulé to combine these ideas into a consistent theory*.   The
theory has been further elaborated by Butlerow (to whom we owe
the name "Chemical Structure"), by Erlenmeyer, and by many
others ; and it has been adopted and applied with slight modifications
by almost all chemists engaged in organic research.

According to this theory the typical form of chemical action is
what we may call the *chemical exchange.*   To illustrate this idea we
may consider the simplest case, that of double decomposition,
where two molecules act on one another to produce two new
molecules.

Chloride of sodium, for instance, acts on nitrate of silver, produ-
cing chloride of silver and nitrate of sodium.   Comparing chloride
of sodium and chloride of silver, we at once see that, while there are
important respects in which the sodium and the silver differ as to
the nature of their union with chlorine (thus the amount of work
required to separate the metal from the chlorine is very different in
the two cases), still from one point of view (and that is the point of
view taken by the atomicity theory) the silver may be said to re-
place or to be substituted for the sodium.   In the same way, a cup
filled with mercury is very different from the same cup filled with
water ; and the relation of the mercury to the cup differs in many
respects (such as pressure and adhesion) from the relation of the
water to the cup ; but they agree in this, that the cup is *filled* in
both cases.   In the same way, the chlorine is said to be *saturated*
by the sodium or the silver, although the intimacy or firmness of the
combination is not the same in the two cases.

We may also consider this double decomposition from the other
side.   As the silver and sodium have changed places, so the chlo-
rine has changed place with the *rest* of the nitrate of silver, with
what in the nitrate of silver is not silver ; or, representing the action
in symbols ($NaCl + AgNO_3 = AgCl + NaNO_3$), Cl and NO_3 have
changed places.

In this example we have one atom or group replacing one other
atom or group ; but all cases of double decomposition are not of
so simple a kind.   Thus, when water is treated with pentachloride

* It is right to observe that although Kekulé has used this theory with the
most eminent success, both in the explanation of facts already known, and in the
discovery of new chemical relations, he does not exclude the possibility of the
union of compounds with each other to form compounds of a second order.

of phosphorus, we find that *one* atom of oxygen (from the water) replaces, and is replaced by *two* atoms of chlorine from the pentachloride; thus $PCl_5 + H_2O = PCl_3 O + 2HCl$; so that while the two atoms of hydrogen were formerly united to one atom of oxygen and formed with it one molecule, they are, after the change, each united to a separate atom of chlorine and form with them two molecules.

Oxygen, therefore, in this case (and, as far as we know, in all cases) enters into two relations, while hydrogen, chlorine, silver, and sodium only enter into one. In a similar way it has been shown that the different elements have different "atomicities," or enter into different numbers of relations. It is to this "polyatomicity," or *multiple-relatedness*, that the complexity of compounds is due; for it is obvious that by the union together of several multiply related atoms a very complicated structure may be produced.

In the case of a compound containing only two atoms, such as chloride of sodium, there is clearly only one way in which it can break into residues; but a complex substance, containing many atoms, may, and generally does, break in different ways when acted on by different substances; and it is by the study of these ways of decomposition of a substance, and by the study of the ways in which, by means of double decomposition, it can be produced, that we arrive at a knowledge of its structure—that is, of the mutual relation of its atoms.

But the multiple-relatedness of some atoms produces a further complication, producing a kind of chemical action which, while still a chemical exchange, cannot be called double decomposition. In double decomposition we saw that each molecule breaks into residues which change places with the residues of the other molecule, and that this breaking into residues results from the rupture of one or more relations between pairs of atoms. But where we have multiply related atoms, it may happen that such a rupture takes place without a separation of the residues, these being retained in combination by some other relation of their multiply related atoms. To illustrate this we may compare the action of anhydrous potash, K_2O, and of anhydrous lime, CaO, on water.

Using graphic formulæ, we have in these two cases :—

and
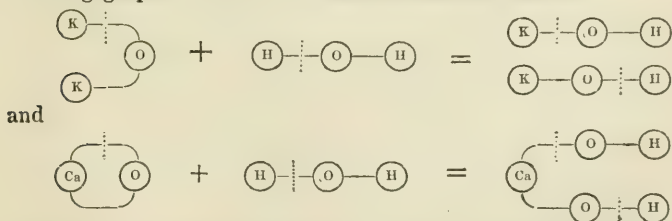
Here the dotted lines indicate the relations ruptured; and it will be seen that, while in the first case the rupture produces a separation into two residues, in the second case it does not, what would otherwise be residues remaining united on account of the double relatedness of the calcium atom.

From this examination of chemical exchange, it will be obvious that no operation of this kind can produce a change in the " atomicity " of an atom ; for every relation ruptured a new one is entered into. But we have no reason to suppose that all chemical action is of this kind ; and there are numerous phenomena which it is very difficult to explain, except by the assumption that there is another kind of chemical action, in which the number of relations of an atom is increased or diminished. Such actions are those by which we pass from one series of compounds to another. Thus the ferrous salts are connected together by processes of exchange ; but it is only by making new hypotheses that we can thus explain the passage from the ferrous to the ferric salts. Similar relations exist between the manganous salts, the manganic salts, the manganates, and the permanganates, where a consideration of each group, apart from the others, would lead us to a different atomicity for manganese ; and many other examples might be given of the same kind. The speaker considered it, in the meantime, to be better to regard each such series separately rather than by an attempt to bring all chemical processes under one class to endanger the stability of the theory of chemical structure, which, while it is probably not destined, in its present form, to remain as a permanent part of the great edifice of the science, is certainly a most convenient scaffolding, not easy to replace, and not hastily to be thrown down.

Having thus seen what is meant by chemical structure, and how we arrive at a knowledge of it by a study of the *history* of the substance, of the ways in which it may be formed and in which it may be decomposed, we may now glance at the relations which exist between the chemical structure of a substance and its physical and physiological properties. We shall consider specially two of the physical characters of matter, volatility and colour, and examine in what way these are modified by the performance upon the substance of certain specified chemical operations. The volatility of a substance depends upon two things :—1st, the temperature at which the substance boils under a particular pressure ; and 2nd, the change of boiling-point produced by a change of pressure. In order, therefore, fully to know the volatility of a substance, its boiling-point must be determined through a very great range of pressure. This involves great labour ; and only a few substances have been thus fully examined. Almost all we know on this interesting question is due to the ingenious and patient experiments of Regnault. These do not, as yet, furnish us with sufficient data to enable us to deduce anything like a law. They show us, however, that a mere comparison of boiling-points under an arbitrarily selected pressure (such as 760 millimetres, which happens to be the mean pressure of the atmosphere) cannot lead us to a law, as the boiling-points of two substances are frequently changed very unequally by a change of pressure.

Such comparisons of boiling-points have been made, and from them have been deduced, especially by Kopp, a series of very interesting and certainly not fortuitous coincidences. That distinguished chemist and physicist has shown that, in a very large

number of instances, the change of chemical structure produces nearly the same change of boiling-point. These "laws" of Kopp are only approximate, and are not even approximate in the cases where the boiling-points of the substances compared are very differently changed by change of pressure.

Turning to the other physical character which has been mentioned, namely, colour, we see at once a marked regularity. As a rule, substances belonging to the same series differ from one another in degree rather than in kind of colour ; while in passing from one series to another, we observe that the colour undergoes a total change of character. This is well illustrated by comparing the colours of substances belonging to such series as the ferrous salts, the ferric salts, the ferrates—the manganous salts, the manganic salts, the manganates and the permanganates—the cupreous and cupric salts, the chromous and chromic salts, the chromates and perchromic acid. Possibly such changes of colour as we see in the transformation of rosaniline and its derivatives into leukaniline and analogous bodies, and of blue into white indigo, may be cases of the same kind. It is also interesting to note that, while the nitro-substitution products of the aromatic series are generally yellow, all the known substances of the same kind in the fatty series are colourless.

These considerations of colour would naturally incline us to regard the operations which lead from one series to another as different in kind from those which lead from one member to another of the same series ; and when we examine the physiological action of bodies of the same and of different series, this impression is greatly strengthened.

The speaker described in some detail a few of the observations made within the last two years by Dr. T. R. Fraser and himself, pointing out the similarity of the action of substances belonging to the same series, and the remarkable change of physiological action produced by those chemical changes which lead from one series to another. The illustrations were drawn from the natural alkaloids (a group of substances containing trebly related nitrogen), and those derivatives of the alkaloids which contain fivefold-related nitrogen. It was shown that the salts of the alkaloids, although containing fivefold-related nitrogen, were not adapted for this comparison, on account of the readiness with which they lose acid in the presence of alkaline substances, their nitrogen thus returning to the trebly related condition. The bodies formed by the addition of a compound of methyl have not this disadvantage ; and as the nitrogen in them is *permanently* fivefold-related, their physiological action may be satisfactorily compared with that of the alkaloids themselves.

The experiments leading to a knowledge of the action of strychnia and of the salts of methyl-strychnium were described ; and it was shown that while the former acts by *exciting* the *origins* of the motor nerves in the spinal cord, the latter act by *diminishing the action* and ultimately paralyzing the *terminations* of the same nerves in the muscles. Similar relations exist between brucia and methyl-brucium, thebaia and the salts of methyl-thebaium, morphia and the salts of

methyl-morphium, &c. Indeed it may be stated generally that, as far as observation goes, compounds of trebly related nitrogen exert an action totally different in kind from similar compounds of five-fold-related nitrogen, that a similar difference exists between the triatomic and pentatomic compounds of other members of the nitrogen family, and that this principle appears to be of still wider, and probably general application.

The speaker, in conclusion, drew attention to the peculiar interest attaching to those regions of science which lie on the frontiers between two distinct departments, as on their successful exploration would depend the ultimate fusion of all physical sciences into one, the science of dynamics, the science which treats of matter and energy and their relations to one another. Such a fusion is probably very remote; but we now see in the border-land between chemistry and physics that slow process of absorption going on which has already converted the once independent sciences of sound, light, heat, electricity, and magnetism into more or less completely subjugated provinces of the great empire of applied mathematics. If we believe in the unity of the plan of creation, we must believe that this process will advance and ultimately triumph.

LVII. Intelligence and Miscellaneous Articles.

ON A MIRAGE IN THE ENGLISH CHANNEL.
BY JOHN PARNELL, M.A., F.R.A.S.*

HAVING had the good fortune to witness an extraordinary mirage at Folkestone on the 13th of April in the present year, I venture to think that the following description of the phenomenon, so far as it passed under my observation, may not be uninteresting.

During the morning, and up to 2 o'clock P.M., a dense fog had hung over the sea; but apparently it was not very deep, as the sun's rays penetrated it pretty freely. At the hour above mentioned the fog opened towards the S.E., disclosing the cliffs on the French coast; and in the course of a few minutes the fog had disappeared, leaving the atmosphere in a state of unusual transparency. The French cliffs were apparently so lofty and with every indentation so clearly visible, that one might easily have imagined that they were but ten miles distant. On examining the objects in view through a small telescope with a 25-power, it was at once apparent that this arose from something more than common *looming*. The French coast could be seen from near Calais towards the E. to far away and many miles beyond Boulogne towards the S.W., the land in the latter direction being ordinarily invisible, as it is situated below the horizon. Immediately under the erect image of the coast was an inverted one, of about double the height of the former. The light-house at Cape Gris Nez gave five images in a vertical line:—the lowest erect but somewhat magnified; above that and separated from it a pair of images of the centre and highest portion of the building only, one erect and the other inverted; and over these another pair, the inverted image being like the former one, but the

* Communicated by the Author.

erect image showing the whole building. Over Boulogne, in the air, were two images of the double funnels and the mast of a tug-boat, the lower image being erect and the upper inverted, the two lines of smoke bending, the one upwards and the other downwards, and both towards the W., till they joined together. The only tug-boat near Boulogne at the time, so far as I could learn, was in the harbour. The cathedral was plainly visible, but only gave a single image. Towards the S.W., and beyond the French coast, some fishing-luggers were observed hull down, so that the position of the horizon could be ascertained ; up to 3 o'clock they presented no un-usual appearance ; but over these were pairs of images of vessels which ordinarily would have been invisible. In some instances three and even four pairs could be observed placed in a vertical line, the lower image in each pair being inverted. With the exception of the uppermost pair, the images seemed to represent the maintop-gallant sail only, and that considerably elongated ; but the highest erect image showed the mizen- and the fore masts and the jib, but in no instance could the hulls be seen. In all cases the inverted images were of about twice the height of the erect. Soon after 3 o'clock vessels between the observer and the horizon began to be affected. The Varne light-ship, which is about $8\frac{1}{2}$ miles from the English coast, had her mast-flagstaff and stanchions elongated to some three times their proper length : this effect lasted for about ten minutes, when they shrank to less than half their usual size, and the hull began to rise till it was nearly as high as it was long, and formed a most conspicuous object even to the naked eye. I then looked towards Dover : the pier seemed completely disorganized ; it appeared to be divided in half longitudinally, with the sea in the midst, and the stone coping moved as if huge waves were agitating it. A steam-boat entering Dover harbour was shrunk to less than half her proper vertical dimensions, but elongated horizontally. Captain Paull, of the S.E.R. steam-boat ' Napoleon III.,' crossed the Channel between the hours of 2 and 4 ; and he told me that he saw Beachy Head during the passage, a circumstance which had never previously occurred during the many years that he has been on the Folkestone and Boulogne route.

At 4 o'clock the atmosphere had returned to its normal condition. The place of observation was about 30 feet above high-water mark. The barometer on the day in question stood at 30·33 in., ther-mometer 58° F. ; wind S.W., very light at 2 o'clock and dropping to a calm ; the electricity of the air unusually high, 185 by a Thomson's portable electrometer (with which five cells of Grove give a potential of 3); and the ozone was 7 of Negretti's scale.

Hadham House, Upper Clapton.
April 20, 1869.

ON THE PRODUCTION OF A BEAUTIFUL PATINA ON BRONZES IN LARGE TOWNS.

In almost all large towns, especially in those in which coal is used as a combustible, it has been found that bronzes exposed in public places, instead of becoming coated with a patina (*vert antique*), have

a dark and dirty appearance like that of cast iron. The desire to remedy this led the Berlin Verein zur Beförderung des Gewerbfleisses to cause comparative experiments to be made with a view to find, if possible, a remedy.

First of all the question had to be solved, whether the production of a beautiful patina is dependent upon the composition of the bronze. For this purpose ten samples of bronzes, remarkable for their beautiful patina and taken from different places, were analyzed. Each of the samples was divided into two, and intrusted to two different chemists. The results were given in the Proceedings of the Verein for the year 1864. They showed that the bronzes investigated were of very varying composition. The proportion of copper varies between 94 and 77 per cent. The quantity of tin amounts in one to 9 per cent., in others to only four ; while others, again, only contain 0·8 per cent., but up to as much as 19 per cent. of zinc. The other accidental admixtures, such as lead, iron, nickel, vary in like manner. With the most different composition these bronzes have all a very beautiful green patina. The composition may possibly have some influence on the time requisite for the production of the patina ; but the experiments leave no doubt that it ensues even with the most different composition.

To ascertain the effect of other influences, a number of bronze busts were placed in a part of the town in which particularly unfavourable exhalations take place, and in which various bronze statues in the neighbourhood are destitute of any trace of patina, but have the above-mentioned unpleasant black exterior.

It was observed that, on several monuments in places accessible to the public, the parts liable to be handled had, if not a green, an otherwise beautiful patina, while all the other parts were black and unsightly. This led the Commission to the idea that grease probably influenced the production of the patina. Hence one of the busts was rinsed every day, with the exception of rainy days, and was, moreover, once a month painted over with bone-oil, which was immediately rubbed off with woollen cloths. Another bust was washed daily with water, but was not oiled. A third, which was also cleaned daily with water, was only oiled twice a year. The fourth was left unpurified, and, indeed, was not at all touched.

The first- and the last-named bust have been set up since 1864, and treated in the manner described ; the second and third since 1866. The supposition as to the action of the fat has been most completely confirmed.

The one which has been oiled once a month possesses a dark green patina, which is considered to be very beautiful by all connoisseurs. The one which is only rubbed twice a year does not look so well ; and that only cleansed with water has none of the beautiful appearance which bronzes obtain by the deposit of patina. The one which was not at all cleansed is quite dull and black.

Hence we may expect that if a bronze exposed in the open air, after being purified, be rubbed down with oil, it will assume a beautiful patina.

How far this rubbing, which in many cases will be difficult to

perform, may be restricted is to be decided by continued experiments which have already been commenced. The Verein has also exposed two bronzes which have been artificially patinated by chemical means, in order to ascertain how they are influenced by similar treatment.

How the oil works in the formation of the patina, cannot with certainty be stated. Experiments have shown that any excess of oil is to be avoided, and that which has been painted on must be rubbed off as much as possible. If excess of oil remains, the dust deposits on it, and the bronze acquires an unsightly appearance. It cannot be assumed that the residual small quantity of oil enters into a chemical combination with the layer of oxide, especially as bone-oil acts just as well as olive-oil in the experiments. Probably the film of oil only acts in preventing the adherence of moisture, by which dust adheres, gases and vapours are absorbed, and in which vegetation forms. Whatever be the mode of action, the experiments leave no doubt that the fat is essentially connected with the formation of the patina.

It is probable that in other respects it will be advantageous. It has been observed that bronzes covered with a beautiful patina, in those parts where water trickles down assume a white, opaque, chalky surface, which in course of time is more and more washed away by the water. Proper treatment with oil will doubtless prevent the formation of these chalky places; yet only long-continued experiments can settle this point.

In any case this use of oil justifies the hope that for the future we may retain beautifully patinated monuments even in large towns. Where coal is the only combustible, they will not be bright, but dark green, and perhaps even black; but they will have the other beautiful property of the patina, the peculiar transparent condition of the surface.—Poggendorff's *Annalen*, April 1869.

TYNDALL'S COMETARY THEORY.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN, Streatham Hill, April 8, 1869.

Professor Tyndall's interesting cometary theory, which appeared in your last Number, turns on an assumption which is hardly compatible with the laws of motion. His assumption is that the tail of a comet is "matter precipitated on the solar beams traversing the cometary atmosphere." If so, the linear dimensions of a comet must exceed the length of its tail—that is, in some cases 60 millions of miles or more. Now it seems utterly incredible that such a bulk of vapour can whirl *round* the sun at the perihelion passage unbroken. A very slight modification of Tyndall's theory avoids this difficulty, and equally well explains all the observed appearances. If there be an extremely attenuated solar atmosphere extending considerably further than the earth's orbit, and if, when a comet approaches the sun and is therefore exposed to intense heat, its volume becomes comparable with the volume of the sun, the heat of the sun will be shut off from that portion of the solar atmosphere in the shade of the comet (which though transparent to light is opaque to heat), and actinic

clouds will be formed in the *solar atmosphere*, thus giving the appearance of a tail to the comet. If the shape of the comet be irregular there may be more than one tail. The comet after its perihelion passage recedes from the sun and at the same time gradually cools and contracts; the tail therefore ought to be greatest and brightest at or shortly after the perihelion, and then slowly diminish and fade away. This is in accordance with observation.

<div align="right">Your obedient Servant,
ERNEST CARPMAEL,
Scholar of St. John's College.</div>

GENTLEMEN,

In the last Number of the Philosophical Magazine Professor Tyndall has propounded the theory that the visible head and tail of a comet is an actinic cloud, resulting from the decomposition of vapour by the solar light. This theory accounts for the extreme tenuity of comets, their polarization, and the motion and development of their tails.

Now if all our knowledge of comets were derivable from observations with the unassisted eye, this theory would account for the observed phenomena; and, as it is, it is valuable by increasing our knowledge of matter which, if not cometary matter, at all events has striking analogies with it; but I do not think it affords any explanation of cometary phenomena as observed by the telescope.

In the first place, immediately behind the nucleus, where, according to this theory, we should expect a very luminous region, we commonly have a dark space. Also the matter which forms the tail usually streams out from the head *towards* the sun; this was especially noticed in Halley's comet in 1836. Bond also, speaking of Donati's comet, says, " the material, after being thrown off from the nucleus, instead of at once being driven into the tail, formed a dense cloud of nebulosity into which the luminous matter continued for some time to stream. *This cloud extended itself on the sunward side, remaining in its vicinity for several days.* When it had acquired a certain stage, the discharge took place mainly from the corners or cusps on either side in two streams, which, coalescing with those issuing from other envelopes, formed the two branches of the tail." Then, too, we have several series of envelopes which rise up *towards the sun*. In Donati's comet seven were detected; in the great comet of 1861 no less than eleven were noticed: the force which causes the ascent of these is intermittent and finally dies away. Mr. Webb, in 1861, noticed the descent of the envelopes on the nucleus; also Herschel and Schroeter noticed the same in the comet of 1811.

From these facts I am disposed to think that although Professor Tyndall's hypothesis accounts for some of the phenomena, yet the true theory is still to be discovered.

<div align="center">I am, Gentlemen,</div>

Talfourd Road, Peckham,   Your obedient Servant,
    April 18, 1869.   W. B. GIBBS, F.R.A.S.

LVIII. *On the Spectra of certain Gases in Geissler's Tubes.* By A. Wüllner*.

I. *Hydrogen.*

THE spectrum which hydrogen enclosed in Geissler's tubes, under very small pressure, exhibits when the current from a small Ruhmkorff's coil passes through it consists, as Plücker first showed†, essentially of three bright lines, which he has designated as H α, H β, H γ. H α is a brilliant red line which takes the place of Fraunhofer's line C; H β is a bright bluish-green line exactly corresponding to the dark line F; and H γ a bluish-violet line which corresponds to a delicate dark line just in front of G; this M. Angström‡ also designated as belonging to hydrogen. Besides these there is a further violet line H δ, the position of which, owing to its feeble light, cannot be accurately determined.

Besides these bright sharp lines there is seen in many tubes a feebly bright field traversed by dark lines, in the neighbourhood of the sodium-line in the place corresponding to the dark line D of the spectrum. This bright field begins about 0·8 of the space between α and D in front of D, and extends beyond D so far that the part on that side is about half as broad as that on the other side of D. Looked at with a flint-glass prism of 60°

* Translated from Poggendorff's *Annalen*, December 1868.
† Pogg. *Ann.* vol. cvii. pp. 506 & 518.
‡ Pogg. *Ann.* vol. cxxiii.

refracting angle with great dispersive power, and through a tele-
scope of about twelvefold magnifying-power, this field is resolved
into about from fourteen to sixteen bright dark-edged and beau-
tifully shaded bands, which are red, orange, yellow to yellowish
green. With different hydrogen-tubes this part is seen of differ-
ent degrees of brightness, so that Plücker originally ascribed it to
the last traces of air still present in the tubes.

Plücker subsequently recognized that this part of the spectrum
also belonged to hydrogen. In an investigation made with M.
Hittorf on the spectra of ignited gases and vapours*, they
show that several elements, as nitrogen and sulphur, have two
different spectra—a continuous one with shaded bands, and one
consisting of bright lines separated more or less by dark spaces;
these are designated by them as spectra of the first and of the
second order. In this investigation they describe this bright field
as a special spectrum of hydrogen of a peculiar character. Plücker
says there, in reference to this spectrum :—" In an old spectrum-
tube which contained very rarefied hydrogen, the ground against
which the three characteristic lines stand out is not always of the
same degree of darkness; sometimes new and brilliant lines appear,
especially near the sodium-line. We confirmed the existence of
a new hydrogen spectrum, which corresponds to a lower tempe-
rature, but exhibits no similarity to all the other spectra of the first
order, those of nitrogen, sulphur, &c. In this spectrum, of a pe-
culiar character, we observed, when it was fully developed, a great
number of well-defined lines, almost too numerous to be counted
or drawn, but bright enough to be investigated with a magni-
fying-power of 72."

That this spectrum is peculiar to hydrogen was further con-
firmed by passing the discharge of a Ruhmkorff's apparatus
through a tube $\frac{1}{3}$ to $\frac{1}{4}$ inch wide, which contained hydrogen
under a pressure of 5 to 10 millims.; a bluish-white line was
then observed in its axis. Analyzed with the prism, it exhibited
the spectrum in question, especially numerous bright lines be-
tween red and yellow. Along with these lines neither H α nor
H γ appeared, only H β, but feebler than many other lines. When a
Leyden jar with an increasing charge was interposed, all the lines
were brighter, H β most so, H α appeared beautifully, H γ more
feebly.

2. About two years ago I observed a third additional spectrum
of hydrogen, which neither in its appearance nor in its formation
agreed with those already described; for especially in its green
part the spectrum has decidedly the character of one of the first
order, and at first I considered it belonged to nitrogen, from
which, however, more accurate comparison showed that it differed

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* Plücker and Hittorf, Philosophical Transactions, 1865, part 1.

essentially at the blue end which is so characteristic of nitrogen. The first observation, which I communicated in 1866 at the May meeting of the Niederrheinische Gesellschaft für Natur- und Heilkunde, was an accidental one. In an experimental investigation on the relation between the refractive indices and the densities of bodies, I used in determining the refractive indices the spectrum of ignited hydrogen consisting of the three bright lines. On this occasion a hydrogen-tube, which had previously been used for frequent measurements, became one day suddenly altered : its previous beautiful red light became white ; the violet line disappeared in the spectrum ; and instead of the former spectrum a continuous one was formed which, especially in the green, was most beautifully shaded. The first supposition, that the tube had begun to leak and had admitted some air, was seen to be erroneous on making a comparison with the spectrum of nitrogen, and in further experiments which had for their object a more accurate investigation of the spectrum ; for the tube just as suddenly assumed its previous red colour, and exhibited as spectrum the three well-known bright lines only. Hence it was established that, under circumstances which as yet were not determinable, hydrogen could furnish a continuous spectrum.

Shortly afterwards I obtained from Dr. Geissler of Bonn a number of new hydrogen-tubes. One of them was found on investigation to undergo just such a change ; it suddenly became white, and yielded the continuous spectrum observed in the first tube. This tube, which is an ordinary one with platinum electrodes, still shows this spectrum when the induction-current merely passes through it. The spectrum is so rich and beautiful in its shading that it is difficult to describe, and still more difficult to delineate*.

At the least refrangible end it exhibits the red line H α, next to which is first of all a perfectly dark field, then, at about 0·15 of the distance H α–D from H α, the continuous spectrum, which reaches from there not quite to H γ—that is, not quite to Fraunhofer's line G. In the red and yellow part about eighteen beautifully shaded more or less broad bands may be discriminated, individual ones of which are remarkable for their brightness. In the bright bands a quantity of very fine lines may be recognized ; so that possibly by using a greater number of prisms, as Plücker has done, this part would be resolved into individual lines, and accordingly coincide with the second hydrogen-spectrum described by MM. Plücker and Hittorf.

* Illustrations of this and of the other hydrogen-spectra as well as of those of aluminium, after drawings by Dr. Bettendorff, are given in the *Festschrift der niederrheinischen Gesellschaft für Natur- und Heilkunde zur 50jährigen Jubelfeier der Universität Bonn.* Bonn : Marcus, 1868.

To the reddish-yellow part succeeds, first of all, a feebly illuminated green field bounded on the right by a brighter green band. Then follows, as far as the line H β, which always remains visible, a beautiful green multifariously shaded field, which by its shading and the bright bands standing out from the darker background, four of them being particularly bright, makes quite a stereoscopic impression. The character of this part is so like that of the green in the first nitrogen-spectrum, that only a closely detailed comparison makes the difference perceptible.

From H β on the blue side, the spectrum reaches to about the middle of the space between H β and H γ; and here also against the faint blue ground four brighter bands stand out, of which the second and the fourth are the brightest. H γ can only be occasionally faintly seen.

I have since then observed just the same spectrum in a tolerable number of other hydrogen-spectrum-tubes when, after being for a long time used with a well-working induction-apparatus, they were illuminated by a feebler current.

The observation of MM. Plücker and Hittorf, that continuous spectra of the first order belong to a lower temperature than the spectra of the second order (consisting of individual bright lines), led to the supposition that this spectrum also belonged to a lower temperature, which might result from the circumstance that, after lengthened use (perhaps through partial melting of the electrodes), the induction-current on entering the gas met with a greater resistance. If this supposition were correct, the method already adopted by Plücker and Hittorf for producing the second spectra (the simultaneous interposition of a Leyden jar into the current of the induction-apparatus) must also again produce the spectrum of hydrogen consisting of the characteristic lines. This supposition was so far confirmed that, on introducing a Leyden jar, the above-mentioned spectrum-tube exhibited a flickering, alternately red and white light, and that, viewing the flickering up of the red light with the spectrometer, the well-known three lines were observed.

The change of the continuous spectrum into that consisting of the three lines was effected with the aid of Holtz's machine. If the current of such a machine without a condenser was allowed to pass through the hydrogen-tubes, their light appeared white, and, viewed with the spectrometer, the continuous spectrum appeared, though faint, yet distinct, especially in the green. If the condenser was laid upon the machine, so that the spark traversed the tube in individual discharges, the light of the tube at once became red and exhibited only the characteristic lines. The same was of course seen when, by means of a Holtz's machine, the discharges of a small Leyden jar with a small striking-dis-

tance were allowed in quick succession to pass through the tube. By these discharges the resistance in the tube appeared even to be permanently diminished; for shortly afterwards the simple induction-current of the small Ruhmkorff, passed through, again produced the red light of hydrogen, which, however, again changed into the white one with a continuous spectrum when the current had passed for about a quarter of an hour through the tube.

3. After it had thus been established that the light of ignited hydrogen, according to its different temperatures, which depend on the mode of discharge, might be different, it was to be expected that there would be a similar difference when, with the same discharge, the density of the gas in the tube varied. To test this conclusion I made some experiments in conjunction with Dr. Bettendorff, which not only confirmed it, but also gave a new spectrum quite different from the preceding ones..

The arrangement of the apparatus was as follows:—In front of the slit of the spectrometer a spectrum-tube was fixed, which near each electrode was provided with a small lateral tube that could be closed by means of a Geissler's stopcock. The upper of these lateral tubes was connected with the horizontal arm of a Sprengel's pump* constructed by Dr. Bettendorff; the lower one was connected with an apparatus for decomposing water, a tube filled with anhydrous phosphoric acid and a bulb-apparatus containing concentrated sulphuric acid being interposed. Over the platinum electrodes of the apparatus for decomposing water tolerably wide glass tubes provided with stopcocks were placed, which at the same time served as gasometers. In preparing the hydrogen, the oxygen was allowed to issue from the tube over the positive electrode, so as not to contaminate the hydrogen with oxygen dissolved in the water†.

After the tube and the pump had been dried by heating and the continued passage of air, the spectrum-tube was exhausted and then filled with hydrogen from the decomposition-apparatus, again exhausted and filled, and this repeated until, at a pressure of from 8 to 10 millims., only the characteristic lines of the hydrogen-spectrum could be seen on the passage of the current. The tube was then again filled with hydrogen, and the light produced by a Ruhmkorff's coil investigated while the tube was

* Journal of the Chemical Society, S. 2. vol. iii. p. 9.

† The necessity for this precaution was indicated by the observation that the spectrum of the hydrogen-tube exhibited oxygen-lines when the oxygen was collected and thus the acidulated water saturated with oxygen was allowed to press out of the tube surrounding the anode and mix with the other liquid. The absorbed oxygen which then, diffused throughout the entire apparatus, escaped over the cathode was quite enough to produce in the spectrum the most distinct oxygen-lines.

being gradually exhausted, the pressure of the enclosed gas being read off on the mercurial column of the pump.

In this way it was found that the current of the small induction-coil can, under a pressure of 135 millims., strike across the tubes, the electrodes of which are 1·4 decim. apart; the tube shines with a white light, the intensity of which is too small to analyze it prismatically.

When the pressure of the enclosed gas is diminished, the brightness of the light steadily increases, and under a pressure of 100 millims. it is bright enough to be investigated with the prism. Under this pressure the light of the tube is bluish white, and sometimes it appears for a moment reddish-tinged. It gives a continuous spectrum, upon which, when the light appears reddish, the bright lines H α and H β distinctly stand out.

Under a pressure of 70 millims. the light of the gas is reddish white, and in the spectrometer the continuous hydrogen-spectrum is seen as above described. It shows first of all H α; as far as half the distance between H α and D the field of view is dark; then follows a reddish-yellow to a greenish-yellow part, consisting of a series of beautifully shaded bands; then another dark one, and then as far as H β a feebly bright green field, on which four brighter green bands stand out. Behind H β, as far as H γ, the field of view is illuminated by a feeble blue light, in which two bands stand out more brightly—the first at 0·3 of the distance H β to H γ behind H β, the second in the middle between these lines.

If the pressure be further diminished, the light in the tube increases in brightness, the colour becomes redder, and the spectrum continually more beautiful. Even under a pressure of 52 millims. the spectrum is developed just as has been already described; it becomes brighter and more beautiful as the gas is still further rarefied, until the pressure amounts to about 30 millims., at which the spectrum appears extremely brilliant.

On further rarefaction the brightness of the continuous spectrum decreases, while the three hydrogen-lines become continually brighter. Under a pressure of 21 millims. the lines H α, H β, H γ were very beautiful, the reddish-yellow part of the continuous spectrum also beautiful, but the green part of the spectrum much enfeebled, only the bright bands in them being visible. Near H β, towards the more refrangible side, only the two brightest lines could be recognized.

Under a pressure of 10 millims. the reddish-yellow part could still be seen in its brightest bands; in the green, individual bands were still faintly indicated; beyond H β scarcely a band was visible.

Under a pressure of 6 millims. the reddish-yellow part was just

visible, besides the characteristic hydrogen-lines; at two places in the green a bright glow was to be seen; and also between H β and H γ the two previously mentioned bright bands were visible.

On further rarefaction to 3 or 2 millims. the characteristic lines retained the same brightness, and everything else disappeared almost entirely from the spectrum; yet, with a simultaneous enfeeblement of the bright lines, part of the continuous spectrum reappeared in the green, in the form of about five bright fields, when the gas was rarefied to fractions of a millimetre pressure. These observations show that the hydrogen-spectrum described belongs, in fact, to a lower temperature than that consisting of the three lines; for with increasing density of the gas in the spectrum-tube the temperature must become lower, since the induction-current experiences a greater resistance in the denser gas, and a larger quantity of the gas has to be heated. But, just as a great density of the gas does not permit the full intensity of the current to be developed, so the current is also enfeebled by great rarefaction; for by adequate rarefaction the current in a spectrum-tube may be completely stopped. The occurrence, therefore, of the continuous spectrum both with greater density and with greater tenuity of the gas proves that it belongs to a lower temperature.

4. By the experiments communicated in the preceding, the proof has been furnished that the continuous hydrogen-spectrum belongs to a lower temperature; and the question arises how it happens that Geissler's spectrum-tubes, which contain hydrogen under a pressure of from 5 to 10 millims. (a pressure favourable, therefore, for producing the line-spectrum), yet after some time, after lengthened use, may yield the continuous spectrum. The observation that this spectrum is especially seen when, after lengthened use, the tube is exposed to the action of a feebler current, led to the supposition that (possibly owing to a superficial melting of the electrodes) greater resistance was offered to the passage of the induction-current. That such a fusion of the electrodes has an influence of that kind was established by a series of experiments. In the extreme degrees of exhaustion mentioned above, and which are to be subsequently discussed, the resistance in the spectrum-tubes was so great that the whole of the positive electrode became incandescent. It thereby became quite bent, and after some time was partially fused, so that it looked like a series of small pearls on a thread. After this deformation of the electrodes had occurred, the tube, on gradual exhaustion of the hydrogen, always exhibited the continuous spectrum, even under pressures at which the green light was otherwise scarcely visible. The continuous spectrum was extremely bril-

liant under a pressure of 30 millims., 21 millims., 16 millims.;
and even under a pressure of 8 millims. it was doubtful whether
the continuous part was really fainter, or whether it only ap-
peared so in comparison with the dazzling lustre of the line H β.

Several observations which we made on a phosphorus-tube and
a sulphur-tube, prepared by Dr. Geissler, favour, I think, the view
that the nature of the electrodes exerts an influence on the pro-
duction of the continuous spectrum.   These tubes also contained
hydrogen.   If the induction-current was allowed to pass without
heating the tubes to the melting-point of phosphorus or of sul-
phur, they exhibited a beautiful continuous spectrum exactly as
has been previously described; the spectrum of phosphorus or
sulphur only appeared on stronger heating.   In these tubes the
electrodes are always more or less covered with phosphorus or
sulphur, by which, since these substances do not conduct, the
resistance to be overcome is greater, and therefore the intensity
of the current must be less.

5. It has already been mentioned, in § 3, that when the pres-
sure of the gas in the tube only amounted to fractions of a milli-
metre the continuous spectrum of hydrogen was again formed,
particularly in the green.   If the tube was then still further
exhausted by means of the Sprengel's pump, the light in the
tube first became feebler and its colour paler; and in the spec-
trum, while all the rest was weakened, the green part stood out
still more beautifully.   It appears in the form of six beautifully
shaded bright bands, which are connected with each other by less
bright intermediate spaces.   In the brightest parts of the blue
in the continuous spectrum bright fields also appear, of which
that in the middle between H β and H γ is seen as columnar
grouped lines.

On further pumping, the light in the tube suddenly becomes
of a splendid green like the light of a thallium-flame, and the
spectrum is quite changed; the red line H α can scarcely be
seen; the reddish-yellow part of the spectrum has completely
disappeared; and in the green six splendid groups of lines appear
on an almost black ground.   Repeated measurements gave for
the least deflection of these groups the following values[*]:—

(1) Middle bright line of the first group, consist-
ing of three bright lines, this being the brightest $\Big\}$ 62° 47′ 40″

(2) Middle line of the second group, consisting
of three . . . . $\Big\}$ 63 10 15

* The flint-glass prism used for these measurements has a refractive
angle of 60° 2′ 00″; its refractive indices are, for H α 1·743355, for
H β=1·772210, for H γ=1·790564: the measurements were made with a
Meyerstein's spectrometer with a circle divided to 10″.

(3) Second brightest line of the third group, consisting of two bright lines . . . . . . } 63 29 20

(4) First bright line of the fourth group, consisting of two very closely adjacent lines . . } 63 46 25

(5) Middle line of the fifth group, consisting of three bright lines: this line is the brightest, and is more than a slit in breadth . . . } 64 22 20

(6) Middle bright line of a group of at least six individual lines . . . . } 64 38 40

(7) H β still faintly visible 64 51 10

These groups, as is evident, corresponding to the green colour of light, all lie in the green part of the spectrum; in the red and yellow part there is nothing to be seen. Besides these measured groups, at the boundary of the green towards the yellow a feebly bright part was seen, between the first and second groups two feebly bright lines, and between the fourth and fifth groups about three faint lines. About as far to the right of H β as the sixth group is to the left of β, there is a feebly bright line, too obscure, however, to be measured. Then at about 65° 20′ there is a faint blue field bounded on both sides by two beautifully shaded bright bands; and behind this, after a perfectly dark space about half as broad as the field just mentioned, there is a faint field of considerable breadth; at times there is in the neighbourhood of H γ, at 67° 10′, a faint lustre.

This spectrum occurs whenever the gas in the tube has attained the extreme degree of rarefaction attainable with a Sprengel's pump. The resistance in the tube is here so great that the positive electrode becomes quite incandescent, bends, and appears to consist of a series of fused globules. That this deformation at the same time seriously hinders the passage of the current, as before mentioned, follows from the fact that on its entrance the current no longer started from the point of the electrode of aluminium wire, but from the part of it which lay against the platinum wire melted into the glass of the tube, where such a fusion could not be perceived.

If the extreme rarefaction which furnishes the spectrum just described is maintained for some time with closed stopcocks, the light of the tube again assumes a white colour and again shows the continuous spectrum, the reddish-yellow part is again seen, the six groups of lines again disappear, and the green appears once more. But the density of the gas in the tube is not changed; for if the stopcock be opened which connects the tube with the air-pump, the position of the mercury remains quite unchanged. Notwithstanding this, renewed pumping again produces the line-spectrum.

Another means of again evoking the line-spectrum is the si-

multaneous interposition of a Leyden jar in the circuit of the
induction-current. A brilliant bright-green light is then ob-
tained in the tube, and the six groups of lines become truly
splendid. At the same time the parts lying between the groups
of lines become brighter, yet not to such an extent as to change
the character of the spectrum. The tube then afterwards ex-
hibits the same spectrum even without a Leyden jar.

This spectrum is also obtained with the extremest degree of
rarefaction if the spark of Holtz's machine with a condenser be
caused to pass, or if a small Leyden jar with a short striking-dis-
tance be discharged through it. Care must at the same time be
taken not to have the striking-distance too great; otherwise the
calcium-spectrum, or the continuous spectrum of ignited glass
with the dark line D, occurs.

With a tube once arranged I have frequently observed this
spectrum for fourteen days together and compared it with others;
so accurately closed were Dr. Geissler's glass stopcocks.

6. The phenomenon of a hydrogen-tube at its extreme ex-
haustion furnishing a third spectrum essentially different from the
earlier ones observed is so surprising, that the question must be
considered whether this spectrum belongs in fact to pure hydro-
gen, or is not due to other elements standing in connexion with
the tube. It might be believed that it was a spectrum of alumi-
nium, of which the electrodes consisted, or of mercury, of which,
perhaps, vapours had distilled over into the tubes, or of phos-
phorus or sulphur, as the gas was dried with phosphoric and sul-
phuric acids, or, finally, that some of the fat with which the
stopcocks were slightly coated had evaporated and had entered
the tubes, and thus that the lines belonged to the spectrum of
carbon.

As regards carbon, the description which Plücker gives of the
spectra of this element[*] shows that none of the groups of lines
of the kind described occur in them; and in an investigation of
the spectrum in a tube filled with carbonic acid, we found it of
the same character as Plücker describes.

In reference to the sulphuric-acid spectrum, Plücker[†] states
that it is one of the most beautiful spectra and at the same time
most rich in colour, it consists of bright luminous bands on a
black ground; and he then counts three red, one orange, one yel-
low, four green, nine blue and violet bands. He mentions at the
same time that, using anhydrous sulphuric acid, this spectrum
can only be obtained with the large induction coil. All this
proves that the spectrum described cannot be due to sulphuric

[*] Philosophical Transactions for 1865.
[†] Pogg. *Ann.* vol. cxiii.

acid, even if we were to assume that along with the hydrogen some sulphuric-acid vapour had entered the tube.

Our spectrum cannot be confounded with that of phosphorus or mercury, as follows from Plücker's descriptions, and as we have convinced ourselves by experiments. Phosphorus exhibits in the green only one group of bright lines, about 8′ broad, the most refrangible of which has the minimal deviation of 63° 38′. The mercury-spectrum consists of a series of bands, of which a yellow one is particularly characteristic; moreover, in a tube containing mercury, this spectrum is only formed when the tube is considerably heated.

To compare the spectrum in question with that of aluminium, wires of this metal were fastened to the ends of the wires leading to the induction-apparatus, and the induction-spark was allowed to strike between these, which were placed in front of the slit of the spectrometer. It was then observed that, according to the distance of the electrodes, the aluminium-vapour might have two essentially different spectra. When the distance of the electrodes was only about two millims., the spectrum consisted of four green splendidly shaded fields. The fields are brightest on the more refrangible side, and gradually diminish towards the less refrangible side; at distances of 5 minutes these fields are traversed by sharp bright lines, and these give quite the appearance of fluting to the fields. The right limit of these fields was found, in the minimum deflection, at :—

						Distance.
1st fluting	.	.	.	63°	24′	10″	
2nd	,,	.	.	.	64	10	20	0° 46′ 10″
3rd	,,	.	.	.	64	54	40	0 44 20
4th	,,	.	.	.	65	40	00	0 45 20

The measured distances of the fields (that is, of their right boundaries) are so nearly equal that their differences may be regarded as errors of observation, since the adjustment is not perfectly accurate; the breadth of the fields is about 30′; so that this spectrum of aluminium consists of four equidistant almost equally broad groups of flutings.

When the distance of the two wires between which the sparks passed was increased to 10 millims. and more, a totally different spectrum was obtained, both with the use of the small Ruhmkorff and with that of Holtz's machine with superposed condenser; the four fluted fields disappeared, and instead of them a number of bright lines and groups of lines started out upon a feebly illuminated background. With the minimum deflection the positions of these lines were :—

(1) Beautiful bright double line . . . . . . 62° 42'
(2) Position of the first of three faint lines, of } 63 0
     which the second is nearer the first than the third }
(3) Bright band 63 51
(4) Bright line 64 17
(5) Bright band 64 22
(6) Faintly bright line 65 5
(7) Middle, brightest, of an entire group of lines. 65 47
(8) Bright line 66 41
(9) Faintly bright line 66 47

The two aluminium-spectra stand in the same relation to one another as the spectra of the first and second order which MM. Plücker and Hittorf have represented for nitrogen, sulphur, &c.; the formation of the second at a greater striking-distance and with the use of a Holtz's machine with a condenser proves that it belongs to the higher temperature.

A comparison of this second aluminium-spectrum with the hydrogen-spectrum described in § 5 shows that they are quite different. Hence it must be assumed that the spectrum consisting essentially of the measured six groups of lines is peculiar to hydrogen.

7. The spectra of hydrogen described in the preceding are essentially different. One is not formed from the other by the development of new lines or new colours as the temperature rises; but quantities of light disappear which are present at a lower temperature, or on a previously continuously illuminated space bright lines stand out at a higher temperature on an almost black ground; for, from the mode of formation of the spectra, it cannot be doubted that the continuous spectrum belongs to the lowest temperature, since the spectrum consisting of three lines, as well as that consisting of six groups of lines, takes the place of the continuous one when the discharges of a Leyden jar are passed through the gas, from which undoubtedly a much greater heating results than from the simple discharge of an induction-current. These spectra may, it is true, be formed by the simple current of induction, but only under circumstances which favour such an increase of temperature. The first spectrum is formed if the gas has such a density that the current is best and most completely developed—the six-group spectrum if only minimal quantities of gas are present for the conduction, which can then be raised to the highest temperature, like the particles detached from the carbon-points in the electric light.

This difference of temperature must, in the case of hydrogen, be regarded as the sole cause of this phenomenon; for a decomposition into further elements is not to be thought of. It follows,

then, from these observations that the emissive power of a substance may materially alter with the temperature.

II. *Oxygen.*

8. The spectrum which a Geissler's tube filled with pure oxygen gives consists, according to Plücker's* description, of a
series of bright lines, the least refrangible of which lies in the red-
orange, and which, more or less close, extend thence to the violet.
Plücker described only this one spectrum, and in the subsequent
investigation, made in common with M. Hittorf, obtained only
the same. In communicating these experiments he says† :—
" We obtained only one spectrum of oxygen working in the
same manner as with nitrogen, with this difference, that under
the same circumstances an equally brilliant spectrum was only
obtained with a stronger discharge. He further states that,
especially in oxygen, the gradual appearance of the bright lines is
noticeable—that at first the least refrangible show themselves, and
at last as the temperature rises the most refrangible come out—
that a drawing, therefore, which represents as simultaneously
appearing those lines which are only successively formed, gives an
ideal picture of the spectrum rather than accords with nature.

The experiments previously communicated on the spectra of
hydrogen, the observation that the spectrum may materially alter
with the density of the gas and the mode of discharge, led me to
investigate oxygen in this direction.

The method of experiment was the same as that described in
§ 3. The lateral tube of such a spectrum-tube as is there described was connected with the tube placed over the anode of the
apparatus for decomposing water. In order to dry the oxygen
supplied, a tube containing phosphoric acid and a bulb filled with
strong sulphuric acid were interposed. The production of a perfectly pure oxygen-spectrum, however, is almost always attended
with great difficulties ; the three characteristic lines of hydrogen
were almost always observed, arising from the moisture condensed
on the inside of the spectrum-tube. It could, however, be expelled by strongly heating the spectrum-tube and then repeatedly drawing pure oxygen through it. When the tube had thus
been dried so that with oxygen under a pressure of from 5 to 10
millims. it no longer showed any hydrogen-lines, it was filled with
oxygen under the ordinary pressure, then gradually exhausted
by the Sprengel's pump as described in § 3, and the spectrum
investigated which the gas gave at different densities.

9. It was first found that, using a spectrum-tube of exactly the

* Pogg. *Ann.* vol. cvii.
† Philosophical Transactions for 1865, part 1, p. 23.

same dimensions as had been used for the experiments with hydrogen, the density of the gas had to be much smaller to allow the current of the same induction-coil to pass through when the same number of elements were used. While hydrogen allowed the current to pass even under a pressure of 135 millims., several experiments showed that with oxygen continuous passage only took place when the pressure was diminished to 45 or 47 millims. The light is whitish, but far too weak for a spectrum-investigation; this was only possible when the density was diminished to 28 or 30 millims. The light appears even then whitish-coloured. The spectrum presents six bright lines—a red line (the flesh-red one characteristic of oxygen and designated by Plücker O α), two green and two blue lines, and a violet one. The brightest have then the following positions of least deviation:—

O α, the flesh-red line	. . .	61 51 30
The second green one	. . .	63 28
The violet	. . .	67 20

After a longer passage of the current, the second red line indicated by Plücker is recognized.

When the pressure is diminished to 25 millims. the tube still shines with a whitish light, the brightness of which, however, has greatly increased. Besides those previously observed, there are seen in the spectrum three fainter green lines, at about 64° least deviation. Under a pressure of 18 to 20 millims. the colour of the light somewhat passes into violet, the brightness is increased; there appear besides the former lines two orange-coloured ones, two very faint yellowish green, a faint blue, at about 65° 10', and a faint violet line at the end of the spectrum.

The pressure being diminished to 6 millims., some new lines occur—a bluish green, and a violet which is somewhat further deflected than those previously mentioned.

At the same time the background on which the lines stand out no longer appears quite dark, but here and there distinctly continuously illuminated. Such a continuous field probably forms the background against which the previously mentioned three faint green lines at about 64° are formed.

Without these lines disappearing, the continuously illuminated fields stand out more distinctly when the pressure is further diminished; and when it only amounts to fractions of a millimetre, the background of the spectrum has become changed into a spectrum of the first order, consisting of several beautifully shaded fields. The colour of the light has become more green; and in accordance with this the continuously illuminated parts lie in the green and in the blue.

The first, very faint yellowish part appears just on the right

of O α. Separated from this by a dark space, a green field is seen, sharply defined on the left, and gradually shading off on the right, among the yellowish-green lines which appear under a pressure of 20 millims.

On the right of the bright-green line at 63° 28′, and separated from this by a dark space, there is a splendid green field consisting of several beautifully shaded bands. It has a similarity to the violet part of the nitrogen-spectrum, inasmuch as its individual parts are brightest and most sharply defined on the left, while on the right they gradually shade off. The field extends to the third of the above-mentioned three faint green lines. Then follows a beautifully shaded blue field, the left limit of which is at about 64° 56′, which continues in strongly shaded parts into the violet.

On continuing the rarefaction, when the pressure can no longer be measured the character of the spectrum suddenly changes, just in the manner described for hydrogen : the continuously illuminated fields disappear ; and in their places, or near them, splendid groups of lines stand out. These perfectly sharp bright lines on a dark ground lie preferably in the green and blue, corresponding to the bluish-green colour of the light which the tube emits.

The spectrum observed does not agree with that described by Plücker, and drawn on plate 2 in the Philosophical Transactions for 1865 ; for the groups of lines are in other positions than those given by Plücker ; and where Plücker draws entire groups of lines, nothing or only individual lines are met with. The spectrum shows five groups of lines, the first two of which are the brightest and broadest. The first of these groups is just in the middle between the positions corresponding to H α and H β of hydrogen, and extends from 63° 11′ to 63° 20′. The second group (the broadest of all) extends from 63° 48′ to 64° 9′; it thus lies about in the middle between the first group and H β. The third group has a breadth of 6′, its middle is at 64° 42′. Then follows, separated by a feebly bright field, a narrow group consisting of five lines, the right limit of which is at 65° 4′. The fifth, very narrow group is at the beginning of the blue, at 65° 40′. In the violet only three lines appear, at 66° 44′, 67° 2′, 67° 8′ 30″; and then a very faint line at 67° 36′, which bounds the spectrum on the most refrangible side.

10. The phenomena described are seen in the above order, if the current of the small induction-apparatus is passed through the spectrum-tube filled with oxygen. In that case the last-mentioned spectrum is obtained when, after the appearance of the continuous spectrum, it is attempted still further to exhaust the tube. The line-spectrum is more easily obtained by adopting

the method proposed by Plücker for producing spectra of the second order—that is, by connecting a Leyden jar with the induction-apparatus; the continuous spectrum then passes at once into that consisting of groups of lines.

11. The best means, however, of investigating the two new spectra is Holtz's machine : by its means the continuous spectrum is obtained without the bright lines of the oxygen-spectrum described by Plücker; and it can thus be demonstrated that the spectrum consisting of lines is essentially different from the continuous one—that is, that the latter spectrum emits light which is different from that emitted by the former.

If the current of a Holtz's machine without its condenser be passed through the tube filled with extremely rarefied oxygen, the light has a sea-green colour, and in the spectrometer only the continuous spectrum is seen without bright lines. Besides a faint reddish field, four beautiful bright fields are first seen, which are sharply bounded and are brightest on the less refrangible side, and gradually shade off towards the more refrangible side, so that the limits cannot there be sharply defined. For the position of the less refrangible limit, numerous measurements, which only differed by fractions of a minute, gave the following values :—

(1) Boundary of a yellowish-green field . . 62 50 30
(2) Boundary of a green field 63 49 00

This field has the greatest brightness.

(3) Boundary of a greenish-blue field . . . 64 56 00

The brightness of this field decreases pretty rapidly in the first quarter; the following three quarters are almost of the same brightness. The breadth of the entire field is about 40'.

(4) Boundary of a blue-violet field which on the } 66 18 40
more refrangible side shades off most beautifully }

Besides these four fields, a few other less bright ones are seen, which could only be partially measured, and partly were estimated in their position relatively to the brightest fields.

Between the red field at O α and the first green one two narrow yellowish-green bands were seen, at 62° 25' and 62° 35'.

Between the fields called above (1) and (2) there is from about 63° 20' a feebly bright field, which also is brightest on the less refrangible side, and gradually shades off towards the more refrangible side.

At 64° 15' there is a feebly bright field about 10' broad, shaded off in bands.

From 65° 52' to the bright field (4) the field of view is feebly

illuminated with a brightness which only slightly diminishes towards the more refrangible side.

A very feebly illuminated field appears then at 66° 54′; it is of small breadth.

12. If the condenser be placed upon Holtz's machine, the continuous spectrum changes at one stroke into the line-spectrum : groups of lines stand out in places which were previously dark ; the bright fields split up; and on the field (2), for instance, bright lines start out right and left of the brightest part, while the brightest part itself becomes dark.

The colour of the light becomes bluish green.

The positions of the individual groups of lines are, from several concordant measurements, the following :—

1st group of lines, left boundary . . . . 63° 11′ 20″
  ,,   right boundary 63 19 30

The right boundary is formed by a very bright line about 3′ distant from the preceding.

2nd group of lines, left boundary 63 47 30
  In the middle a very large bright double line. 63 58 00
          Right boundary . . . 64 8 40

This group is formed from the green field which was before designated as (2) ; it gives the impression that the brightest part has been torn asunder at the left limit and separated into individual lines.

3rd group, of six lines . . between 64° 37′ and 64° 46′

The right boundary is the brightest.

4th group of lines. It starts from the field designated in the preceding section as (3), yet in such a manner that the brightness of the lines increases towards the more refrangible side ; the brightness has thus a distribution the reverse of what it has in the continuously illuminated field. The left limit of this group cannot, therefore, be quite sharply determined ; several measurements furnished values between 64° 58′ and 65° ; so that the beginning of this group does not coincide with that of the bright field. The right boundary is at . . . . 65° 4′ 40″

5th group of lines, in the blue, three bright lines . . . . . from 65° 40′ 10″ to 65 44 00

6th. The bright field which, without the condenser, begins at 66° 18′ 40″, disappears entirely when it is added. Instead

of it several lines stand out on each side of this place, which, however, cannot be arranged in groups, and are not of great brightness.

7th. In the violet part of the spectrum there are—

A bright violet line . . . . at 66̇ 45̇ 00̈
A feebly bright group 6′ broad, from 67° 3′ to 67   9 00
A bright violet line . . . . at 67 36 30

If the discharges of a small Leyden jar be passed through a Geissler's tube filled with highly rarefied oxygen, just the same spectrum is obtained; with a stronger charge it becomes more brilliant without otherwise changing.

When the spectrum obtained with a Holtz's machine is compared with that described in § 9 as obtained with the Ruhmkorff's coil, it is at once seen that both are identical, although, owing to the greater brightness in individual groups with the Holtz's machine, a few lines become visible which could not be seen in the former case.

It therefore follows that in this case, as also with hydrogen, three distinct spectra may be obtained with induction-currents, according as the gas in the tube has greater or less density. That this difference in the spectra is solely due to the different temperatures of the gas follows from the experiments with the Holtz's machine.   The same considerations which in § 7 led to the continuous spectrum being regarded as that corresponding to the lower temperature, and that consisting of groups of lines as corresponding to the highest temperature, lead here to the same conclusion.   The continuous spectrum belongs to the lowest temperature (although it is not seen with gas of great density), because it is formed by the continuous discharge of the Holtz's machine.   The spectrum described by Plücker, which with gas of suitable density may also be produced in its essential features with the small Ruhmkorff's apparatus, belongs to a higher temperature.   The last mentioned, which is attained with gas of the least density by the aid of the Ruhmkorff's coil and of a Leyden jar, belongs therefore to the highest temperature.

III. *Nitrogen.*

13. In investigating the spectra of nitrogen, Geissler's tubes were filled with dry air, after what Plücker states had been confirmed, that dry air furnishes the same spectrum as pure nitrogen.   With air in Geissler's tubes no traces of oxygen-lines are seen; and there is here no difficulty in getting the spectrum free from hydrogen-lines; the tube need only be filled a few times with air which has been dried by sulphuric and phosphoric acids.

Using the same induction-apparatus as in the previous experiments, the current just began to pass through the tube filled with air when the pressure was 94 millims; yet the light was not continuous. A continuous passage of the current only occurred when the pressure was diminished to 64 millims, though the luminous intensity was so small that a prismatic investigation of the light was not possible. On a further diminution of the pressure, the brightness of the light gradually increases; and under a pressure of 46 millims. the luminous intensity is adequate for spectrum-investigation. The less refrangible parts of the spectrum in the red and yellow are barely visible; only from the green is the spectrum distinctly present; most beautiful are the violet parts, which are so characteristic of the nitrogen-spectrum.

The red and yellow parts occur first under a pressure of 30 millims.; but they are so faint, that the shaded bands which Plücker has described in the nitrogen-spectrum of the first order are at most scarcely perceptible. The green part with its rich shading stands out more; but the blue and the violet are the most beautiful; in them the individual flutings are completely developed.

Under a further diminution of pressure by 5 millims., red and yellow come out more, and the beautiful shaded bands of the complete nitrogen-spectrum are visible. Under a pressure of 18 millims. the spectrum is completely developed; it quite corresponds to the description which Plücker has given of it *, and to what a spectrum-tube filled with pure nitrogen exhibits.

The brightness and beauty of the spectrum increases as the pressure diminishes; under a pressure of about 5 millims. it is developed most brilliantly, and remains so until the pressure of the gas is less than 1 millim. Only when the pressure is so far diminished that it can scarcely be measured by Sprengel's pump does the brightness become less, the darker parts being first extinguished, and finally only the brightest parts visible. In its appearance the spectrum approximates to one of the second order, without, however, changing into one, for no new bright lines appear.

With a simple Ruhmkorff's apparatus, then, only one spectrum can be exhibited in a tube filled with nitrogen; a difference in density is only of influence so far, that the spectrum is more or less complete and appears of greater or less brightness.

Using, too, a Holtz's machine without superposed condenser, the nitrogen-spectrum of the first order was seen as with an induction-apparatus. Using the condenser or a small Leyden jar, the spectrum of the second order described by Plücker occurred.

* Plücker and Hittorf, Philosophical Transactions for 1865.

Even when the exhaustion had reached its utmost limit the appearance was quite unchanged.

14. Nitrogen thus only furnishes the two known spectra; and without using a Leyden jar the first spectrum cannot be changed into the second. Hence there is a considerable difference between the behaviour of hydrogen, oxygen, and nitrogen. With the first two gases the same mode of discharge can yield entirely different spectra in the enclosed gas, according to its density. Hence this difference can have no other reason than the higher or lower temperature to which the gas has been heated, and which, as mentioned in § 7, depends on the different density of the gas. It must be assumed that the emissive power of both gases does indeed essentially vary with the temperature. The case is different with nitrogen: the difference in temperature produced by the different density of the gas is not sufficient to change the spectrum; the mode of discharge must be changed. Nitrogen can only be brought into the condition in which it yields a spectrum of the second order, by the sudden passage of large quantities of electricity, obtained by simultaneously interposing a Leyden jar in the circuit of the induction-coil, or by passing the discharge of a Leyden jar with the Holtz's machine. Hence we may speak of an allotropic condition of nitrogen, which furnishes the second spectrum, and which is formed by the sudden discharge of large quantities of electricity, which, however, returns to the ordinary form as soon as the temperature diminishes. To be sure, no explanation is thus given of the difference in deportment of nitrogen and other gases; this can only be expected from further experiments, which will be reported upon in due course.

Bonn, August 1868.

LIX. *On the Motion of a Palladium Plate during the Formation of* Graham's *Hydrogenium. By* James Dewar, *F.R.S.E.*[*]

GRAHAM, in continuing his exhaustive researches on diffusion, has recently examined the relation of gases to various colloid septa. The remarkable discovery of Deville and Troost of the permeability of platinum and iron by hydrogen at a red heat, he has expanded into a general examination of the relative rates of passage, at high temperatures, of the various gases through different metallic septa. Further, he has proved that different metals have a specific occluding power over certain gaseous elements, retaining them in combination at low temperatures, although the absorption took place at a red heat. Of

[*] Communicated by the Author, having been read before the Royal Society of Edinburgh, March 1, 1869.

the many astonishing discoveries made during the course of these investigations, probably the most remarkable is the occlusion of hydrogen by palladium. This metal, whether in the form of sponge or hammered foil, when heated and cooled in an atmosphere of hydrogen, absorbed between six and seven hundred times its volume, increasing to the enormous occlusion of 982 volumes when the metal used had been deposited by voltaic action. This occlusion of hydrogen, Graham has shown, can be easily effected at low temperatures by making palladium the negative electrode during the electrolysis of water. He has also shown that the metal charged with hydrogen increases greatly in volume, and that its physical properties are entirely modified. So marked is the change in the physical, electrical, and magnetic properties of the combination, that the only class of compounds we can compare it with are the metallic alloys. In the occluded state the chemical intensity of hydrogen is increased, many reactions being effected by its agency beyond the power of the element in the free state. Graham, as a general result of his experiments, considers the occluded gas to exist in the form of a solid, with all the physical properties of a metal. During the course of an experimental exhibition of Graham's discovery, I noted several phenomena associated with the occlusion of hydrogen by palladium when it is made the negative electrode during the electrolysis of water ; and as they illustrate in a new form the results already arrived at by the Master of the Mint, with his permission I am induced to bring them before the Society.

If a palladium plate, used as the negative electrode during the decomposition of water, be arranged at right angles instead of parallel to a similar platinum plate, the hydrogen in a short time is evolved at the edge of the palladium plate nearest to the platinum electrode, no trace of hydrogen coming from any other part of the plate. Gradually, as the saturation takes place, the hydrogen seems to travel slowly along the plate, and only after saturation is it freely evolved from the whole surface of the electrode. If we now reverse the current, so as to evolve oxygen at the palladium plate, immediately the nearest edge begins to evolve gas, the rest of the plate remaining tranquil ; the evolution of oxygen moves along the plate in a gradual manner. This gradual transference depends on the time necessary to effect the occlusion, and on the relative intensity of the lines of force.

When a palladium plate charged with hydrogen is brought into contact with a platinum electrode freely evolving oxygen, evolution of gas is immediately arrested over the entire surface of the electrode. The same plate, free from hydrogen, when brought into contact with a platinum electrode evolving hydrogen,

shifted the evolution of gas only on the same side on which it was firmly pressed. In order to examine the action of mixed electrodes, the palladium plate was welded with a similar platinum plate into a V-shaped electrode, when the apex of the combination could be placed in or out of the liquid. If the platino-palladium electrode is made the negative pole, hydrogen makes its appearance immediately on the platinum plate; no gas is evolved by the palladium for some time. If, after saturation, by reversing the poles oxygen is thrown on the mixed electrode, no gas is evolved from the platinum; and when the gas began to be evolved, it appeared simultaneously on both plates. The same result is observed whatever may be the position of the compound plate relatively to the other pole in the liquid, relative distance from the other pole having no effect. This proves that the whole of the oxygen that ought to be evolved on the platinum is transferred by a polar chain through the liquid on to the palladium plate, so long as this plate contains occluded hydrogen. The presence of the strong electric current is shown by connecting the hydrogenized palladium with a platinum plate in an acid liquid, the circuit containing a galvanometer. If the apex of the V electrode is placed in the acid liquid, additional phenomena are witnessed, depending upon which side of the compound electrode is next to the other electrode. If the platinum side of the V electrode is firmly clipped in a stand, a glass rod keeping the apex in the same position in the liquid, and if the palladium plate is next to the positive electrode, we observe the following change during the course of the hydrogenation. The angle of the V continually diminishes by the motion of the palladium towards the perpendicular, the hydrogen evolved coming only from the outer surface of the compound plate. After some time the plate returns to its original position, and would curve beyond it if the action were continued. If oxygen is now evolved on the compound plates the first effect of the oxygen is to curve it beyond its first position, or to diminish the angle of the V. If the palladium plate is furthest from the positive electrode, the first effect of the hydrogenation is to increase the angle of the compound plate by the palladium moving outwards; after some time it returns. Similar observations with the palladium clipped in a stand, but made with the junction out of the liquid, showed a decided movement depending on the relative position of the plates. Seeing the palladium moved, although firmly clipped in a socket out of the liquid, it was evident the motion could be examined without the use of compound plates.

After devising several arrangements in order to examine with ease the motion of the plate, the following plan was found to be the best in practice :—

The electrodes of palladium and platinum were firmly clamped in the little vice represented by D, and arranged as shown in the
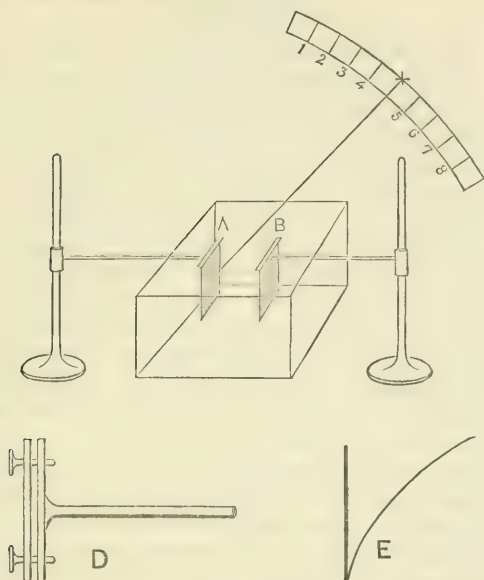
figure, where A represents the palladium and B the platinum. To the lower edge of the palladium plate a narrow strip of the same metal is fused by the oxyhydrogen blowpipe; the strip is of such a length as to project above the level of the acid liquid when the plate is immersed. To the end of the strip of palladium a thread of glass is fused so as to have a radius in all of 40 or 50 centims.; the arc of a divided circle of the same radius on a piece of cardboard is supported by a stand at the extremity of the index. By this apparatus the small deflection of the palladium plate is greatly magnified, and the direction of motion well defined. Suppose the palladium plate A in the figure is connected with the negative pole of the battery, the glass index, after a short time, begins to move from left to right on the plane of the diagram to the extent of 8 or 10 centims. on the scale. As the saturation goes on, the index begins to move backwards from its first position, going towards the left, even to a greater extent than its first deflection towards the right. Continuing the action, it again returns to near its original position. The power of being able to return to the position it had at starting seems to depend on the condition of the plate, as regards the

distribution of tensile strain produced by rolling ; at least after repeated use the plate lost the power of returning after having passed towards the left.   If the plate after saturation is connectd with the positive pole of the battery, the first effect on the index is to move quickly towards the left, then to return to where it was, this double motion taking place before any gas makes its appearance on the palladium.   If the platinum electrode B is placed on the opposite side of A and the saturation of the plate repeated, the index goes through the same series of positions, but the direction of motion is reversed.   The direction of motion depends, therefore, on the relative positions of the electrodes, but is constant for the same position.   This is easily shown by allowing the index to commence its motion, say, from left to right ; then by moving the positive electrode to the other side of the palladium plate, the motion immediately commences in the op- posite direction, although the saturation was far from being com- plete.   The motion of the index when oxygen is thrown on the hydrogenized plate depends also on the position of the electrodes. The index has also a motion at right angles to the plane of the scale, the resultant motion being compounded of the separate flexures of the plate.   Many other devices could be used to show the motion, such as a plate bent into the form of a cylinder with a narrow channel left between the two edges, which would shut and open alternately, or *vice versâ*, according to whether the po- sitive electrode were without or within the cylinder.

Graham has shown that the formation of the alloy of hydro- genium and palladium is attended with an enormous increase in the volume of the metal.   He found that a wire of palladium 100 millims. in length became $101 \cdot 5$ millims. when saturated with hydrogen.   Now, if a uniform hydrogen atmosphere sur- rounds a symmetrical piece of palladium, there is no reason why it should penetrate with a greater rapidity one surface rather than any other.   But if the absorption is not uniform on all the surfaces, from want of uniformity in the hydrogen atmosphere, the surface absorbing must produce a flexure of the plate from the expansion of the metal.   If a thin plate of this rigid metal can be so arranged as to induce absorption on one side rather than the other, then, as a necessary consequence, the plate will become convex on the side where the greatest relative absorption is taking place ; and as the saturation approaches uniformity, the convexity should disappear, the plate regaining its original form if the elasticity of the metal is not changed during the action.   A plate of palladium, when it functions as the negative pole during the electrolysis of water, is subjected during the course of the action to the supposed non-uniformity of the gaseous atmosphere, if the surface of the plate is parallel to a similar

platinum electrode. The amount of chemical action effected by the unit-surface of the electrode, and its distribution, depend upon the position of the plate relatively to the lines of force emanating from the poles. Now, of the two surfaces of the palladium plate, the surface next to the positive pole has the greatest concentration of the lines of force; and therefore the greatest evolution of hydrogen takes place on this surface, so that the quantity of alloy formed in the first instance is in excess of the amount produced on the other side. The plate, from the great expansion on the outer side, becomes convex, until the progression of the action on the other side is able to compensate this flexure by a corresponding expansion, thereby bringing the plate to its original form. But I have already explained how this backward motion goes beyond the original position, producing a flexure in the opposite direction, only in some cases returning to its normal shape. This, in all probability, arises from the combined effect of expansion and compression. Graham has shown that the tenacity of the alloy is considerably diminished as compared with that of the palladium, so that in the first flexure of the plate the expansion has to compress the rigid palladium, whereas, when it begins to return, the compression is effected on the less rigid hydrogenium already formed on the other side, the elasticity of form being also probably relatively small. This is the general explanation of the motion of the plate; but it must be remembered that the smaller flexures produced by different forms and positions of the electrodes are the result of non-homogeneity or excessive strain. There is one position, at least, of the palladium relatively to the other electrode, where the plate should have no lateral motion—that is, when it is at right angles to the surface of the positive plate; but all the attempts made with the small palladium plate at my disposal failed to prevent lateral motion, probably from want of uniformity of surface producing different rates of penetration.

Effect on the Current during the Formation of the Hydrogenium.

In order to determine the effect on the current during the occlusion of the hydrogen and its reoxidation, two Bunsen's cells were connected, through a tangent-galvanometer, with a plate of palladium and platinum as poles in a cell containing acidulated water. The intensity of the current was determined, first, during the decomposition of the water when the palladium pole evolved oxygen, then during the absorption of hydrogen (when the current was reversed), and lastly when the oxidation of the occluded hydrogen was taking place.

	Palladium Positive pole.	Palladium Negative pole.	Hydrogenium Positive pole.
Angles on tangent-galvanometer ...	$23\overset{\circ}{\cdot}5$	$21\overset{\circ}{\cdot}5$	$40\overset{\circ}{\cdot}3$
Tangents	0·4348	0·3939	0·8541

The diminution in the intensity of the current during the formation of the hydrogenium arises from the strong current generated in the decomposing-cell by the secondary polarity of the electrodes, acting in the opposite direction, whereas the oxidation of the hydrogenium produces a current acting in the same direction as the battery. The intensity of the current during the oxidation of the hydrogenium is nearly doubled; so that if we consider the resistance in the circuit to remain constant, the additional electromotive force added by the oxidation of the hydrogen is equivalent to two cells of Bunsen. The great increase in the intensity of the current during the oxidation of the hydrogenium may be shown in the following manner:—Use two palladium plates in the decomposing-cell with index to show the motion of the plate attached to each, as in the figure formerly given, and include in the circuit a commutator and a fine spiral of platinum wire. During the occlusion of hydrogen the platinum spiral may be made of such a length as to remain dark, the motion of the index proving the absorption of hydrogen. Reverse now the direction of the current, and the spiral of platinum will become red-hot, the index moving rapidly back to its original position, while the index of the other plate will begin to move. The oxidation of the occluded hydrogen is the limit to the brightness of the platinum spiral, so that a reversal of the current produces a renewed brightness. By this arrangement we keep both indices moving along the scale, and the platinum spiral alternately bright and dark. If the electromotive force is really equal to a Bunsen's cell, this arrangement must produce a current far more intense than any similar gas-battery where oxygen and hydrogen are the reacting elements; but as the resistance in the circuit might vary, several indirect experiments were made. When a palladium plate saturated with hydrogen was associated with a platinum plate in an acid solution of permanganic acid and connected through a voltameter, the arrangement could decompose water. A similar result was obtained when a platinum plate coated with platinum-black saturated with oxygen was employed; but the action in this case was very slow, and sometimes did not succeed. A platinum

plate covered with peroxide of lead was opposed to the hydroge-nium ; the combination decomposed water with facility. Grove found that two pairs of his gas-battery could effect a slow decom-position of water, and a single pair did as well if the oxygen was replaced by peroxide of lead. (The intensity of the palladium and peroxide-of-lead combination must render it particularly well adapted to form secondary piles of great intensity, by substitu-ting it instead of a lead plate in the arrangement devised by M. G. Planté.) The transformation of gaseous hydrogen into the occluded state would seem to have little effect in reducing its total chemical energy, so that the occluded hydrogen must re-tain a relatively large proportion of the total gaseous energy in a potential form. If we compare the occlusion of gases to work done on the gas, the elaborate researches of Joule and Thomson on the " Thermal Effects of Fluids in Motion " would lead us to believe that hydrogen, of all gases, would in this new condition retain the greatest amount of its original store of energy. But before a just comparison can be made with the results of Joule and Thomson, careful determinations must be made of the electromotive force, latent heat, &c. of hydrogenium. Professor Tait has determined by a new process the electromotive force of platinum and palladium covered with oxygen and hydrogen ; and the result will be communicated to the Society in a short time.

LX. *Fundamental Principles of Molecular Physics.*
By Professor J. Bayma, *S. J., of Stonyhurst College.*

[Concluded from p. 358.]

PROFESSOR NORTON convinced as he was that he had fully answered all my objections and that his fundamental principles required no further vindication, felt himself free at last to ponder over my shortcomings, and proceeded directly to criticise five items of my molecular doctrine, viz. the curve of molecular action, the production of heat and light, the transmis-sion of rays, the constitution of molecules, and the transmission of force.

Curve of molecular action.—The learned Professor begins thus :

" The attempt to deduce the existing constitution of things and prominent phenomena by Professor Bayma from his fundamental ideas, so far as made, has certainly failed at several important points. To specify one or two of these. He obtains a curve of molecular action that represents a repulsion at the smallest distances succeeded by an attraction at greater distances. This can only be made to

represent the three states of bodies by conceiving the molecules of a
gas to be in such a condition that, if it were entirely freed from
pressure, it would expand into a liquid.   We know that many gases
can be compressed into a liquid, but it is altogether gratuitous to
suppose that they could be brought into a similar condition by a di-
minution of pressure.   Experiment has given no indication of such
a result or tendency."

This is a mistake.   My curve of molecular action does not
require the molecules of a gas to be in such a condition that, if
entirely freed from pressure, it would *expand into a liquid*.
What I said is this.   " If hydrogen, or any other gas, were freed
from pressure, it would expand as much as its constitution re-
quires, viz. to a certain determinate bulk only ; for there must
be a limit of expansivity for all permanent substances.   Then
it would remain at rest in the same manner as if it were a liquid.
Its molecules would then be at the distance of equilibrium : and
its difference from known liquids would only be that, whilst
other liquids do not allow themselves to be sensibly reduced in
bulk, hydrogen would allow of a very sensible reduction " (Mo-
lecular Mechanics, p. 195).   Such is my real doctrine.   But
Professor Norton seems to have thought that a thing, which can
remain at rest *in the same manner as if it were* a liquid, must
needs be a liquid : and it is on the strength of such a brilliant
discovery that he proceeded to pronounce that I have " certainly
failed " in this first of several " important " points.

But, again, my curve of molecular action in his judgment
" cannot represent the three states of bodies," unless the
gaseous molecules are of the condition already described.   I
reply that my curve of molecular action has been drawn from
the consideration of bodies in general, without descending to
those special conditions which must be verified in the case of
gases.   Hence Professor Norton is perfectly free to remove,
even indefinitely if he likes, the intersection of the curve with
the axis, viz. the point where repulsion ceases and attraction
begins.   In this case the actions represented by the ordinates
of the curve will easily become repulsive all throughout to his
full satisfaction : only it might be difficult, after this change,
to say what body in nature we could point out as possessing
the properties indicated by the curve so boldly stripped of its
inferior branch.

As for the limit of expansivity of the gaseous molecules, its
necessity is not gathered from the curve of molecular action, nor
from the nature of the gaseous state, but from the permanency
of the gaseous substance.   That it is not deduced from the
curve of molecular action is quite evident—since the curve is
only the expression of facts already established, and represents

under a general aspect a law of action already recognized. The nature of the gaseous state, on the other hand, is in no need of a limit of expansion; for if we grant, out of deference to Professor Norton's opinion, that the molecules of hydrogen instead of having a limit of expansion can expand indefinitely, what in the world would prevent such hydrogen from being a highly gaseous substance? Surely, it would not cease to be an expansive gas for the sole reason that its expansivity would have no limit. The only reason why primitive gases have a limit of expansion is an *à posteriori* reason, viz. because they are permanent substances, having a constitution which cannot be destroyed by external action whether mechanical or chemical. Such substances, though expansive, must have a limit of possible dilatation; otherwise they would tend of themselves to their own dissolution; in other words, they would cease to be permanent. (See Molecular Mechanics, p. 145.) This is the reason why I have admitted that a gas freed from all pressure would expand to a determinate bulk only, and then remain at rest in the same manner as if it were a liquid. Professor Norton cannot hold the contrary, unless he holds either that primitive gases are not *permanent* substances, or that their molecules, though permanent, must repel each other at all great distances. The first hypothesis would lead to the conclusion that the gaseous molecules have no sufficient constitution of their own, but depend for their existence on all sorts of exterior agencies: which view is too unscientific to be adopted by the learned Professor. The second hypothesis would entail the impossibility of any chemical combination of gases; for if the gaseous molecules, though permanent, repel each other at all great distances, their absolute power of repelling is certainly greater than their absolute power of attracting: and thus chemical affinity between two gases will become an impossibility. This suffices to show that Professor Norton's criticism on this first "important point" is wholly without foundation.

Heat and light.—My second "failure," according to Professor Norton's statement, regards the origin of heat and light. He says:

"Heat and light he conceives to originate in vibrations of gross molecules; but against this notion, as I shall take another occasion to show, insuperable objections may be urged. If this be given up, his explanation of the changes of the state of bodies must also be abandoned."

I have already stated somewhere that I conceive heat *to consist* rather than *to originate* in vibrations of molecules. Of course, I do not say in vibration of "gross" molecules; because "gross

matter " has nothing to do with my conceptions, and is the ex-
clusive property of my learned critic, as I have made clear on a
previous occasion. If however by gross molecules he means
molecules of ponderable bodies having the constitution which I
pointed out in my treatise, then I allow (though protesting
against the epithet " gross ") that the heat of such bodies *con-
sists* in vibrations of such molecules. I do not know just now
what " insuperable objection " the learned Professor will urge
against this view on another occasion : when those objections
arrive, we may think of them. Meanwhile an objection which
my opponent, I fear, will not solve is the following. The
learned critic, while defending his three forms of matter, de-
clared emphatically that my repulsive envelope is essentially his
" electric æther," and consequently that my repulsive envelope is
not his " gross matter." If therefore in my theory the mole-
cular envelope is the sole agent directly concerned in the com-
munication of heat from molecule to molecule, it is evident that
heat in my theory does *not* originate in the vibrations of gross
matter. Now such is exactly the case. He may read in my
treatise that " calorific motion is communicated from molecule
to molecule mainly through their respective envelopes, which
are in the best condition for strongly influencing one another.
The rest of the molecular masses, *i. e.* the nuclei, move *in con-
sequence* of the motion to which the respective envelopes have
been subjected, according to the nature of the molecular con-
stitution ; and therefore the difficulty of communicating calorific
motion to a body does not depend, except in a very secondary
degree, on the inner part of the molecules (Molecular Mechanics,
p. 205). This *inner part* is what my critic assumes to be
" gross " matter. So long as this objection remains unanswered
I have reason to question the exactness of Professor Norton's
statement and the conclusions drawn therefrom.

And now with regard to light. Is it true that I conceive
light " to originate in vibrations of gross molecules ? " I need
not repeat that gross matter has nothing whatever to do with my
conceptions. What I had said was this : "*Luminous* bodies
cannot excite luminous undulations of a certain period in the
surrounding æther, unless they themselves move at the same
period and make as many undulations. With regard to *non-lu-
minous* bodies, they either transmit or reflect light ; and there-
fore although they are incapable of setting themselves into spon-
taneous vibrations suitable to make a sensible impression on our
organ of vision, yet *they are prepared, when acted upon by impin-
ging rays, to take up the same kind of vibratory motion*, at their
surface at least, if they are opaque " (Molecular Mechanics,
p. 209). This view we are now going to examine.

Transmission of rays.—My critic says :

" The doctrine that transparent bodies transmit rays of light by the motion of their own molecules, will hardly be accepted, we think, by physicists."

The author knows, I assume, that Mr. Grove, who is an eminent physicist, holds in his ' Correlation of Physical Forces ' that the phenomena of light can be explained without any intervention of æther, and consequently by the vibratory motion of ponderable matter. And although I think, as I have remarked in my ' Molecular Mechanics,' that Mr. Grove has failed to establish the non-existence of æther as a special substance, his arguments have still sufficient weight in the case of the transmission of light through solid bodies, as the reader will see in my treatise (pp. 190–192).

The author adds that it would be " a waste of time " to argue against this view. I cannot subscribe to his opinion, nor probably would Mr. Grove. A view unsupported by reasons may be passed over without arguing, as it might be a waste of time even to mention it : but when reasons are adduced, a critic should at any rate mention the fact of their existence and bearing, before he decides that arguing against them is a waste of time : a decision which should rather be left to the reader himself. The principal reason adduced by me in support of my view was that " in the hypothesis that the ray is transmitted by the motion of the æther intercepted between the molecules, the regular arrangement of these would not explain the fact of the transmission of a ray in any other direction than that determined by molecular interstices in a straight line. Now the ray is in fact transmitted in all other directions. Accordingly we maintain that the transparent bodies transmit the rays of light by the motion of their own molecules, not by the motion of intercepted æther " (Molecular Mechanics, p. 191). Had Professor Norton transcribed this argument, and then declared his repugnance " to waste time " about it, the reader would have been able to judge of the matter : but my opponent did not give the reader any such satisfaction, although, I venture to say, this would not have been a waste of time.

Professor Norton adds :

" The notion that a certain substance radiates light of a certain colour, because its molecules are made to vibrate in unison with the ray of that colour, will not stand ; for the results of spectral analysis show that the parts of a body which are capable by vibration of giving out any colour are precisely those which absorb and stifle that colour. This fact, we may add, also proves conclusively that the rays cannot be transmitted by the motion of the molecules."

I confess that I cannot see the conclusiveness of this proof : first because the fact referred to regards incandescent substances only, whilst the molecules to which the author alludes in this conclusion are not supposed to be incandescent ; and therefore he has not the right of applying to the second the law of the first phenomenon : secondly because spectral light is a light elicited from the substance which is being burnt or made self-luminous, whilst the light simply transmitted is a light received from without ; and it is obvious that what is truly said of a self-luminous body with regard to its own light cannot be said truly of a non-luminous body with regard to a light which it receives from without. These two reasons flow from one fundamental fact that heat alters, at least for a time, the molecular constitution of bodies by causing a change in the relation of molecular radii, an increase of vibratory motion, and a modification of molecular distances. Such alterations evidently cannot take place without a proportionate modification of the optical properties of the body. Thus, for instance, hyponitric acid is almost *colourless* at the low temperature $-20°$ C., becomes *yellow* at the temperature $0°$ C., *orange* at $15°$ C., and *intensely red* when transformed into vapours. It is evident therefore that the period of the luminous vibrations depends on the temperature of the body and changes with it. Accordingly the period of the vibrations of an incandescent body and the quality of the spectral light radiated by it while existing in such a violent condition cannot be assumed to indicate the period of vibrations and the quality of the light transmitted or reflected by a body existing in opposite conditions.

Professor Norton concludes in the following words :

" This fact . . . . though so radically at variance with Professor Bayma's theoretical views, is in entire accordance with my own. For, according to these, light originates in certain vibratory movements of the atoms of the electric atmospheres of molecules, and when these vibrate naturally in unison with the ray of any colour that falls upon them, they take up its *vis viva,* and so the ray is transformed into a molecular electric current."

The reasoning expressed in this passage comes to this. A substance when ignited and incandescent absorbs and stifles such and such rays ; therefore such and such rays are always absorbed and stifled by the same substance, even though it be not ignited and incandescent : and the theory which authorizes us to draw such a conclusion is therefore more scientific than any opposite doctrine. The reader will be able to judge for himself of the value of this argument. On the other hand when the atoms of the electric atmospheres of molecules (my molecular envelope) vibrate naturally in unison with the ray of any

colour that falls upon them, and take up its *vis viva,* is it neces-
sary to conclude that the ray will be " transformed into a mole-
cular electric current ? " or is it not more natural to maintain
that the ray will remain a ray and undergo no transformation
at all ? A transformation must have a cause calculated to sub-
stitute one form for another, and therefore to destroy the form
of the ray and to introduce the form of an electric current.
Now the cause which destroys a kind of motion must be con-
trary to that kind of motion : if it were in unison with it, it
would propagate instead of destroying it. And therefore, if
the atoms of the electric atmospheres vibrate in unison with
the ray, we must conclude that the ray is transmitted or pro-
pagated, not transformed into a molecular electric current.
But to proceed.

Elements and molecules.—Professor Norton allows that my
leading principles " may in the main be conceded:" a conces-
sion for which I feel obliged; but then he thinks that such
principles " by no means cover the whole ground upon which
the theory is raised." The thing is quite possible, as my ' Mo-
lecular Mechanics ' contains suggestions and hints which are
not strictly and solely derived from such " leading " principles ;
yet they are few in number, and of so secondary an importance
that I do not consider them as a substantial part of the theory*.
But even so, those suggestions are not gratuitous, and cannot
be discarded without a fair examination of the reasons by which
they are supported.

Professor Norton to prove his assertion points out that I as-
sume " that all elements or material points of the same form of
matter act, under similar circumstances, with the same inten-
sity." I wonder how this can be true, since in my book I ex-
plicitly stated the contrary. I said : " One might ask : Is there
any material element possessing a greater power than any other?
This question cannot be answered in the present state of science.
Still it would be rashness on our part to assume, without neces-
sity or indication of any kind, that all elements have equal
power " (Molecular Mechanics, p. 69). It is strange that my
American critic should have read the opposite, especially when
we consider that all the molecular formulas contained in the
book take into account the different intensity with which ele-
ments can act under the same circumstances. The allegation is
therefore a clear mistake.

Then my critic argues in the following strain :

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* I take this opportunity to acknowledge the impropriety of an example
given in my book (p. 171), where I stated that the sun exercises *no sen-
sible* influence in the production of tides. Professor Haughton of Trinity
College, Dublin, kindly pointed out the inaccuracy of the statement.

" Now, if this principle be admitted, what theoretical basis have we for the existence of distinct primitive molecules for every different substance, the number of elements associated together being exactly the same for each primitive molecule of each substance, and different for primitive molecules of different substances ? The natural tendency would be to a fortuitous association of elements in an endless variety of numbers into groups.   No controlling principle by which uniformity would be evolved from chaotic confusion, is furnished by the theory.   The hand of the Creator must be supposed to have miraculously interfered, and guided each element to its precise place in the formation of every molecule of matter."

I have brought in this passage, though I have no necessity of giving it a reply, only because it shows what style of reasoning is sometimes adopted by men of scientific merit.   Professor Norton starts from the assertion that my elements act, under the same circumstances, with equal intensity : and the assertion is false.   He then draws a conclusion ; which accordingly has no foundation.   He assumes that the conclusion must be rejected in order to give a theoretical basis to molecular science, and does not reflect that the existence of distinct primitive molecules is in no need of a theoretical basis *à priori*, it being quite sufficiently established as a matter of fact by *à posteriori* reasonings. He implies gratuitously that elements must have existed in a previous state of dissociation ; and therefore considers them as having a natural tendency to a fortuitous association, and speaks of chaotic confusion with no controlling principle.   He comes to the consequence that " the Creator must be supposed to have miraculously interfered ;" and seems either to regret the necessity of the fact, or to imagine that in the work of creation there has been nothing miraculous.   Lastly he supposes that the Creator must have interfered " by guiding each element to its precise place," as if the elements could not have been created in those precise places without any need of a miraculous guidance.

To strengthen this first argument, he adds :

" The objection here urged derives still greater force from the consideration that both the nucleus and envelope of each specific molecule are assumed to have a regular geometric form, different for each substance.   To assume the existence of such molecules is to make an incalculable number of arbitrary assumptions."

The reader already knows how in my criticism on Professor Norton's " fundamental principles " I had ventured to say, and to prove, that his theory contained a great deal of arbitrary assumption.   It is not surprising then, that the learned Professor should now and then indulge, to his own relief, in an attempt at retaliation, and that here, at the end of his paper, should make a *décharge générale* of an " incalculable number " of projectiles.

Thus far there is no harm. But is it true that the nuclei and envelope of each specific molecule are " assumed " by me to have a regular geometric form ? The question is one of fact : every one may easily satisfy himself that I in my ' Molecular Mechanics ' have not simply assumed, but proved the regularity of molecules in general, and clinched the demonstration by those symbols Q. E. D., which Professor Norton has so well discerned on a previous occasion (see Molecular Mechanics, pp. 150–152). The critic may discuss my proofs if he likes ; but I deny that he has the right of dissembling their existence.

Again, have I assumed, as the learned Professor says, that the geometric forms of molecules are " different for each different substance ? " This is another question of fact ; and the fact is quite the reverse of the statement. Not only I never affirmed what Professor Norton imagines, but on the contrary I went so far as to maintain that the molecules of oxygen, nitrogen, chlorine, sulphur, iodine, are all of the same geometric form, though they belong to different substances (see Molecular Mechanics, pp. 236–243). Moreover, as there are from sixty to seventy different primitive substances, it was as impossible for me to admit that each different substance had a different regular geometric form, as it would have been to discover from sixty to seventy different regular polyhedric forms.

Transmission of force.—I hasten to say a word about the last " failure " pointed out by Professor Norton in my ' Elements of Molecular Mechanics.' He says :

" We have already seen that the principle that one material point acts upon another instantaneously, without the intervention of any medium, is opposed to the fundamental idea that the force exerted is inversely proportional to the square of the distance. This law, to say the least, is an arbitrary assumption in the premises."

I look in vain for the page in which " we have already seen " what my able critic here advances. What we have seen is only his assertion that " the law of inverse squares is a consequence of wave-propagation ; " but he gave no proof of it *for the case of elementary action,* and I defy him to give one. Wave-propagation is propagation of motion, and regards only the progressive development of a series of effects and of their conditions, not of their causality. Physicists indeed often say that actions are conveyed through a material medium ; but this expression means only that a material medium is indispensable for the progressive development of the aforesaid series of effects, as I have shown in my work (p. 64). As for the " arbitrary assumption in the premises " I need only remark that Professor Norton neither brought forward my premises, nor said in what they consist. The reader will find them in my book

(p. 63), and will soon be convinced that they are not arbitrary assumptions, as advanced by the learned Professor.

He then adds:

"The author also conceives that the mutual action of two material points is in no degree and under no circumstances intercepted by another intervening point.   But we know that, in the case of the molecular forces, the amount of *vis viva* expended in imparting motion to one particle is abstracted from the force in action; and, according to Professor Bayma, the molecular forces are of the same nature as the forces subsisting between the material elements.   The force of gravity, it is true, is not sensibly intercepted; but this does not prove that a tendency to interception does not exist; for upon the supposition of a wave-transmission of the force, the effective attraction of any molecule may be the mere differential of the actual force transmitted, and, besides, in the circular revolution of a planet the distance from the sun remains unchanged."

The statement that in my opinion mutual action is never intercepted by intervening matter is perfectly true: only the writer might have noticed that I never confound *action* with *motion*, and therefore though I hold that action cannot be interfered with by intervening matter, I hold nothing of the sort regarding motion.   Had he made the same important distinction, he would have seen that his objection is a mere sophism. We know indeed that "the amount of *vis viva* expended in imparting motion to one particle is abstracted from the force in action" or more exactly *from the body in motion*; but we do not know that it is abstracted from the active power of the element or from its action.   *Vis viva* is a function of velocity; and velocity is neither action nor active power, as I have shown at length in my treatise (pp. 19–25, also pp. 44, 45).   The body in motion will therefore, by a loss of *vis viva*, lose motion, not action, nor active power.

The other statement, that, according to me, "the molecular forces are of the same nature as the forces subsisting between material elements," may be true or false, according as the word "forces" is assumed to mean active powers or the result of their combined exertions.   In the first case the statement is true, because the action of the whole molecule proceeds from the same powers which constitute the molecular system.   In the second case the statement is false, because the nature of the resultant depends not only on the nature of the active powers, but also on the nature of the actual composition or mechanical state of the molecule: so that the resultant of their actions (which is considered as the action of the molecule) widely differs from the action of each element, and, unlike it, does not follow the Newtonian law at molecular distances, as is well known.

Finally Professor Norton insists that, though the force of gravity is not sensibly intercepted " this does not prove that a tendency to interception does not exist : " and to prove the existence of this tendency he argues from " the supposition of a wave-transmission of the force." To this I reply that nothing proves the existence of the slightest tendency to the interception of the action of gravity or any other action. Such an interception can moreover be demonstrated to be absurd, as there is no power in nature which is calculated to intercept action. As for the " supposition " (the author seems here as elsewhere to accept suppositions as proofs) I reply that it is only a supposition : which moreover has nothing to do with the exertion of active power ; since the propagation of waves is a phenomenon of motion, not of its causality with which alone we are here concerned. But my critic, who employs the indefinite word " force " instead of " action," could not help confounding the exertion of power with the consequent motion and its propagation.

Out of fairness to Professor Norton, I must quote a short passage that I find in one of his articles (Phil. Mag. vol. xxviii. pp. 277, 278) where he gives his reason for admitting the interception of action. He says :

" If two molecules are in equilibrium under their mutual actions, the attractive and repulsive impulses exerted by each upon the central atom of the other must be equal, and *therefore* no effective action, either attractive or repulsive, can be transmitted to other most distant particles on the same line. Under these circumstances, one molecule in receiving the action of another, *intercepts* the action that would otherwise take effect upon other most distant molecules. This being admitted " &c.

I recommend this curious reason to the consideration of physicists, of those especially who deal most with dynamics. As for myself, I have italicized the word *therefore*, but shall not stop now to give a special answer ; as the passage is not taken from the paper to which I intended to reply. On the other hand, the author must himself solve his argument in the case of gravity, which, as he allows, is not sensibly intercepted. To conclude. What I have said suffices, in my opinion, to show that Professor Norton has been unsuccessful both in his answers to my objections and in his arguments against some of my conclusions. The cause of his ill-success lies almost wholly in his too great facility to take a stand upon gratuitous hypotheses and mere conceptions, which he also confounds too frequently with " established truths." This fact is now so evident that I might be allowed to consider his last paper not as an answer to, but as a striking confirmation of, my previous objections.

I cannot end without sincerely thanking the learned author for the honour which he has bestowed on me by condescending to criticise my 'Elements of Molecular Mechanics.' The more so, as his criticism, whilst impugning a few points of secondary importance, contains what to my great satisfaction I consider to be a virtual approbation of my leading principles. Nothing can be more gratifying to the author of a scientific work than to find his fundamental views shared by men of such accomplishments in science as Professor Norton is known to be, whatever unavoidable differences may remain in some matters of detail.

LXI. *On a Metallic Connector to replace the Vulcanite Tube used with* Bianchi's *Air-pump.* *By* WILLIAM SWAN, *LL.D., F.R.S.E, &c., Professor of Natural Philosophy in the University of St. Andrews**.

IN the air-pumps constructed by M. Barthélemi Bianchi of Paris, which are driven with a continuous motion of rotation, the cylinder C (fig. 1), like that of an oscillating steam-engine, vibrates on trunnions, T, U, through a considerable arc. The trunnion T is pierced to form an air-passage, to which a tube (A)
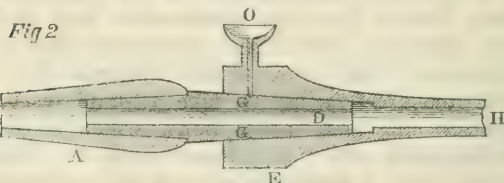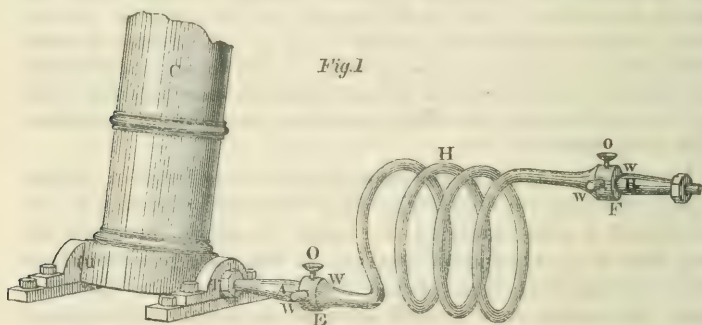
Fig.1

Fig 2

is screwed on air-tight. In order that the cylinder shall be free to oscillate, it is evident that the tube A does not admit of being *rigidly* attached to any fixed piece of apparatus ; and accordingly, to connect A with the tube B communicating with the pressure-gauge and air-pump plate, M. Bianchi employs a flexible tube of vulcanite, which is prevented, by a helix of wire inside, from collapsing with the pressure of the atmosphere when the pump is working.

Such a vulcanite connector, when of the quality furnished by M. Bianchi and when new, answers its end most satisfactorily ; but, like all other vulcanite tubes, in course of time it becomes brittle, and, especially where it is attached to the metal tubes A and B, it begins to leak. Thus it happens, when an experiment is to be made, that the pump, in every other respect in perfect order, may be found to be useless until the vulcanite has had its ends cut off and is again attached, or until it is replaced by a new tube. I believe all who have been in the habit of using this form of air-pump must have experienced the inconvenience thus arising, and will agree with me in thinking that, although the use of a vulcanite tube may sometimes be convenient, the necessity of employing it always is the only objectionable feature of Bianchi's otherwise most excellent instrument.

In order to remedy this defect, I have recently constructed a metallic connector for the Bianchi air-pump belonging to the Natural-Philosophy Museum of the United College, St. Andrews. This consists of a brass tube, H (fig. 1), about 6 feet long, $\frac{1}{4}$ of an inch bore, and $\frac{1}{3}$ of an inch outside diameter, coiled into a helix of three and a half turns about 6 inches in diameter. To the air-tubes, A of the pump and B of the pressure-gauge and pump-plate, are attached, by soldering, tubes with conical ends. That which is soldered into the air-tube A is carefully centered. It is represented at D (fig. 2). The D are fitted air-tight, by grinding, into hollow conical pieces, E, F, attached by soldering to the ends of the connecting helix H. To prepare the pump for working, the cones D having been slightly oiled, the piece E of the connector is slipped on to the cone D of the pump at A ; and the pump-plate and gauge are slid on the table on which they stand towards the pump until the conical tube D at B (not seen in the figure) entering the hollow piece F, takes its seat and slightly compresses the helix H lengthwise. The helix of brass tube, although quite rigid enough from point to point to preserve its shape, yet possesses ample elasticity as well as flexibility to keep the cones D in their places, notwithstanding any jolting motion which may occur, even when the pump, for rapid exhaustion, is driven with the utmost speed. It need scarcely be further explained that the cone D at A turns

freely within the hollow piece E, and thus the cylinder C has perfect liberty to oscillate without *necessarily* twisting the connecting tube H, although, owing to the unavoidable friction between the rubbing surfaces, at each stroke of the pump a slight torsion of the connector actually does take place. In order to ensure the connexions remaining perfectly air-tight, oil-cups, O, are placed on the pieces E, F, and a groove, G (fig. 2), is cut round each of the cones D, so that when a little oil is dropped into the cups it runs down into the grooves and effectually prevents the passage of air. In the apparatus as constructed, wings, W, *w*, were added at the ends of the connector, and also placed on the tubes A and B with the intention of stretching vulcanite bands over the wings to maintain the connexions until such time as the atmospheric pressure, owing to the progress of the exhaustion, should of itself be sufficient for that end. The wings have proved convenient in handling the connector; but it has not been found necessary in practice to make use of vulcanite bands. A pair of stops might be attached to the pump-stand immediately below and in contact with the wings, W, so as effectually to hinder their oscillation. Thus, were it deemed desirable, the slight torsion of the tube H, already noticed, might be prevented. It is indeed quite possible that, failing this precaution, in course of time the tube may become brittle by continued twisting. Not only the wings, but, I believe, the oil-cups also, or at least that at B, might be dispensed with; but I am not disposed to advise that either of the cups be omitted.

The apparatus, as described, which has been exceedingly well constructed by Messrs. Kemp of Edinburgh, was designed by me for use with a moveable air-pump which is placed on the floor, while the pump-plate stands on a table. If the air-pump be permanently screwed to a lecture-table, which, where room can be spared, is a good plan, the apparatus might advantageously be simplified. The helix might probably be made of a shorter tube, and the fastening at B might be permanent. A straight tube might even perhaps replace the helix, or a tube bent at right angles, where the table does not admit of the pump-plate standing in front of the pump. In either case sufficient play might be given to keep the cone D in its place while the pump is working, if the pump-plate were mounted on a board with small castors or rollers; and, if necessary, vulcanized bands might be stretched over the wings on the tubes A, E, to draw them together. With a permanently fixed pump, I would also suggest that, by making the helical connector long enough, the connexions at A and B might be made absolutely rigid and permanent, the oscillatory motion of the cylinder being rendered possible simply by the torsion of the helix. Per-

haps in this construction it would be necessary to employ a tube of tempered steel, but probably one of brass or copper might suffice.

I will only further add that the moveable connector which I have described has been found to work most satisfactorily. It can be mounted or dismounted in an instant; and if at any time it be desirable to revert to the use of a vulcanite connector, all that is needed is to pull the vulcanite over the cones D on to the tubes A and B.

In conclusion I cannot omit the opportunity now afforded me of warmly recommending, with or without the new connector, Bianchi's most elegant and useful air-pump. Owing to the very great rapidity with which, by its use, a vacuum can be obtained, it is particularly well adapted for lecture-experiments. The pump-cylinder and nearly all the apparatus being made of cast iron, is not liable to suffer corrosion from oil, and will therefore, I have no doubt, last much longer than a brass air-pump. I have found a glass case very effectual in preventing external rusting; and the instrument is so beautifully finished as well to deserve such protection. M. Bianchi has adopted in his air-pumps Babinet's ingenious arrangement for double exhaustion. By turning a stopcock, one end of the barrel of the pump is made to exhaust the other; and thus a very perfect vacuum, as is well known, can be obtained.

LXII. *Upon the new Conception of Electrodynamic Phenomena suggested by* Gauss. *By* R. Clausius*.

IN a letter written by Gauss in 1845 to W. Weber, the brief remark occurs that he considered the corner stone of electrodynamics to be *the deduction* of the accessory forces (which are superadded to the reciprocal action of electrical particles at rest) *not from an instantaneous* action, but (as is the case with light) from one requiring time for its propagation. Based upon this remark, three very interesting papers, by Riemann, C. Neumann, and Betti, have lately appeared. All three authors arrive by different ways at the result that the forces which two currents exert upon each other are explicable on the assumption that a certain time is necessary for the propagation of electrical actions.

I have read these researches with the greatest interest, but must confess that I am not satisfied with them; and I think that both the well-deserved reputation which these authors possess in the scientific world, and the importance of the subject, amply justify me in stating my objections. The fact that three distin-

* Translated from a separate impression, communicated by the Author, from Poggendorff's *Annalen* for December 1868.

guished mathematicians, by pursuing three different methods of investigation, have obtained what is virtually the same result, would seem to furnish a guarantee for the accuracy of the investigation ; and perhaps many physicists will thereby be led to consider the matter settled. Under these circumstances a publication of the reasons on the other side can only have a useful effect, by giving rise to further investigations made probably from other points of view.

The most complete of the above investigations is that of C. Neumann, which appeared, under the title "Principles of Electrodynamics," as a publication on the occasion of the Jubilee of the University of Bonn, after a preliminary notice of the results had been published in the Proceedings of the Göttingen Royal Academy of Sciences. I will first discuss this.

Neumann starts from the consideration of two points, which move under their reciprocal action. If the potential of one of these points, (for greater accuracy we will at once say) the point m_1, is to be determined in reference to the point m, the time necessary for the propagation of the action must be taken into account, which is done by Neumann in the following manner.

The potential which the point m_1 exerts at the time t he calls the *emissive* potential, and denotes it by π. Assuming Newton's law in reference to the reciprocal action of the two points, namely that the force is inversely as the square of the distance, we have, according to Neumann, for the determination of the emissive potential the equation

$$\pi = \frac{m\, m_1}{r},$$

in which r denotes the distance of the two points at the time t.

This potential requires some time to reach the point m, and hence arrives there not at the time t, but somewhat later. If, on the contrary, we want the potential arriving at the time t in the point m, which Neumann calls the *receptive* potential, we must take for it that potential which the point m_1 emitted at a certain earlier time $t - \Delta t$. Denoting the distance which the two points had at this previous moment by $r - \Delta r$, and choosing for the receptive potential the sign ω, according to Neumann we must put

$$\omega = \frac{mm_1}{r - \Delta r}.$$

Replacing the denominator by a series progressing by powers of Δt, we have

$$\omega = \frac{mm_1}{r - \dfrac{\Delta t}{1}\dfrac{dr}{dt} + \dfrac{\Delta t^2}{1\,.\,2}\dfrac{d^2 r}{dt^2} - \&c.}. \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (1)$$

As regards the difference of time Δt, it is the time which the potential requires to traverse the distance between the two points; and denoting by c the velocity of propagation of the potential, Neumann puts

$$\Delta t = \frac{r}{c}. \qquad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

The previous equation thereby passes into

$$\omega = \frac{mm_1}{r - \dfrac{r}{c}\dfrac{dr}{dt} + \dfrac{r^2}{2c^2}\dfrac{d^2r}{dt^2} - \&c.}. \qquad . \quad . \quad . \quad (3)$$

Developing this expression in a series, and, inasmuch as c is very great, neglecting all members which have in their denominator higher powers of it than the second, we obtain

$$\omega = \frac{mm_1}{r}\left[1 + \frac{1}{c}\frac{dr}{dt} + \frac{1}{c^2}\left(\frac{dr}{dt}\right)^2 - \frac{r}{2c^2}\frac{d^2r}{dt^2}\right]. \quad . \quad . \quad (4)$$

From this expression Neumann separates that part which may be represented in the form of a differential coefficient according to the time, viz.

$$mm_1\left(\frac{1}{c}\cdot\frac{1}{r}\frac{dr}{dt} - \frac{1}{2c^2}\cdot\frac{d^2r}{dt^2}\right),$$

and calls it the *ineffective* potential. The other members form then the *effective* potential. Denoting this by w, he finally obtains the equation

$$w = \frac{mm_1}{r}\left[1 + \frac{1}{c^2}\left(\frac{dr}{dt}\right)^2\right]. \qquad . \quad . \quad . \quad . \quad (5)$$

In this statement I think I have accurately reproduced that part of Neumann's investigations with which we are concerned for our present purposes, and we will now inquire whether this reasoning can be allowed to be correct in all points.

If the action of the point m_1 on the point m is presupposed to be momentary, both the emissive and the receptive potential of m_1 in reference to m must be represented by the fraction $\frac{mm_1}{r}$, in which r is the distance which the two points possess at the time t. In order to express that a duration of time is needed for the propagation of the potential, and that thus the potential which arrives at the time t on the point m was exerted from the point m_1 at an earlier time $t - \Delta t$, Neumann puts in the receptive potential the value $r - \Delta r$ as the denominator of the fraction instead of r, by which he understands that distance which the points had at the time $t - \Delta t$. But if we assume that the action is propagated similarly to light from the point m_1 to

the point m, it seems to me that we must take as denominator of the fraction neither the distance which the points have at the time t, nor that which they had at the time $t-\Delta t$, but must take into account another quantity—that is, *the distance between the position where the point m_1 was at the time* t $-\Delta$t (at the moment of emission) *and the position where the point m is at the time* t (the moment of reception). This distance may be denoted by $r(t-\Delta t,\ t)$.

To determine this distance, it is convenient, besides the above-used differential coefficients of the distance r, in which the motions of the two points are simultaneously taken into account, to bring in two other kinds of differential coefficients of this magnitude—those, that is to say, which only refer to the motion of the point m, and those which only refer to the motion of m_1. We have now to do with the latter, and will distinguish them by an index added to the d, thus, $\frac{d_1 r}{dt}$, $\frac{d_1^2 r}{dt^2}$, &c. The distance in question may then be expressed by the following series,

$$r(t-\Delta t,\ t) = r - \frac{\Delta t}{1} \frac{d_1 r}{dt} + \frac{\Delta t^2}{1 \cdot 2} \frac{d_1^2 r}{dt^2} - \&c.$$

Putting this value into the denominator of the fraction which is to represent the receptive potential, we obtain instead the following expression,

$$\omega = \frac{m m_1}{r - \dfrac{\Delta t}{1} \dfrac{d_1 r}{dt} + \dfrac{\Delta t^2}{1 \cdot 2} \dfrac{d_1^2 r}{dt^2} - \&c.} \qquad . \quad . \quad . \quad (1a)$$

This expression is essentially different from Neumann's. If it be applied to two constant currents, presupposing that in each of the two conductors there is everywhere an equal quantity of positive and negative electricity, we obtain zero for the potential of the two currents on each other.

Besides what has here been said, there is another objection which I think I must make against Neumann's analysis. To determine the difference of time Δt which the potential requires to pass from the point m_1 to the point m, Neumann uses equation (2),

$$\Delta t = \frac{r}{c};$$

that is, he regards in this determination the distance r, which the two points have at the time t, as the path traversed by the potential. But this is obviously not the real path; this we must again consider to be the above-discussed distance—*that is, the distance of the position of the point* m_1 *at the moment of emission, from the position of the point* m *at the moment of arrival.*

Hence, instead of the previous equation, we must put

$$\Delta t = \frac{r(t - \Delta t, t)}{c};$$

or if we take for $r(t - \Delta t, t)$ the above-mentioned expression,

$$\Delta t = \frac{1}{c}\left(r - \frac{\Delta t}{1}\frac{d_1 r}{dt} + \frac{\Delta t^2}{1\,.\,2}\frac{d_1^2 r}{dt^2} - \&c.\right).$$

From this equation we get for Δt_1 the following series,

$$\Delta t = \frac{r}{c} - \frac{r}{c^2}\frac{d_1 r}{dt} + \&c. \quad . \quad . \quad . \quad . \quad . \quad (2\,a)$$

Reverting to equation $(1\,a)$, if we replace Δt by the series just found, we obtain instead of (3) the equation

$$\omega = \frac{mm_1}{r - \dfrac{r}{c}\dfrac{d_1 r}{dt} + \dfrac{r}{c^2}\left(\dfrac{d_1 r}{dt}\right)^2 + \dfrac{r^2}{2c^2}\dfrac{d_1^2 r}{dt^2} - \&c.}; \quad . \quad (3\,a)$$

and from this we get the following equation instead of (4),

$$\omega = \frac{mm_1}{r}\left(1 + \frac{1}{c}\frac{d_1 r}{dt} - \frac{r}{2c^2}\frac{d_1^2 r}{dt^2}\right). \quad . \quad . \quad . \quad (4\,a)$$

This expression for the receptive potential is distinguished from that in (4), not only by the complete differential coefficients of r being replaced by partial differential coefficients, but also by the fact that the principal member of Neumann's formula (that containing the square of the first differential coefficient) is wanting.

It appears to me to follow indubitably that Neumann's analysis, by means of which he deduces his potential formula from the assumption mentioned in the introduction to his paper (that the potential, like light, is propagated through space with a certain constant velocity), cannot in all respects be admitted as conclusive. Whether perhaps that formula may be arrived at by different assumptions as to the mode of propagation of the potential, or by the help of other points of view, is, of course, still undecided.

I turn now to Riemann's paper, which appeared in Poggendorff's *Annalen*, vol. cxxxi. p. 237 [*], after it had been presented to the Göttingen Royal Society in 1858, but subsequently withdrawn by Riemann.

Riemann endeavours to deduce an expression for the potential of a constant galvanic current S, in reference to another constant galvanic current S′, from the same assumption, namely, that the potential requires time for its propagation through space.

He first of all considers two small quantities of electricity, ϵ

* Phil. Mag. S. 4. vol. xxxiv. p. 368.

and ϵ', moving in the two conductors. The distance from each other he calls r; but from the first he makes the distinction that in determining r he considers the two quantities of electricity at different times. The distance between the position of ϵ at the time t and the position of ϵ' at the time t' he denotes by $r(t, t')$. If thus we wish to determine the potential emitted from ϵ at the time $t - \Delta t$, and which arrives at ϵ' at the time t, we have, as before, to denote by $r(t - \Delta t, t)$ the value of r which then comes into play. Thus the potential of ϵ upon ϵ' (if, with Riemann, we use the minus sign, since similar electricities repel each other) is

$$-\frac{\epsilon\epsilon'}{r(t-\Delta t, t)}.$$

In calculating with this expression, he distinguishes between the differentiations which refer to the motion of ϵ from those which refer to the motion of ϵ', by denoting the first by d and the latter by d'.

For the sake of greater convenience, he introduces the functional sign F with the meaning

$$\frac{1}{r(t, t')} = \mathrm{F}(t, t'),$$

by which the expression for the potential passes into

$$-\epsilon\epsilon'\mathrm{F}(t-\Delta t, t).$$

Using this functional sign, he distinguishes between the deductions which refer to ϵ and those referring to ϵ' by affixing to F either an accent or an index, that is, either by F' or by F_1.

So far nothing can be urged against Riemann's expressions; but in the further treatment points occur with which I cannot agree.

First of all, in determining Δt he proceeds just like Neumann. Denoting the velocity of propagation of the potential by α, he puts

$$\Delta t = \frac{r}{\alpha},$$

in which r is the distance of the quantities of electricity ϵ and ϵ' at the time t. His expression for the potential accordingly becomes

$$-\epsilon\epsilon'\mathrm{F}\left(t - \frac{r}{\alpha}, t\right).$$

But to carry out logically his own distinction of the various distances, he ought to put

$$\Delta t = \frac{r(t-\Delta t, t)}{\alpha},$$

which, neglecting the higher members, gives

$$\Delta t = \frac{r}{\alpha} - \frac{r}{\alpha^2}\frac{dr}{dt}.$$

In accordance with this the expression for the potential should be

$$-\epsilon\epsilon'\mathrm{F}\left(t - \frac{r}{\alpha} + \frac{r}{\alpha^2}\frac{dr}{dt},\ t\right).$$

In the further course of Riemann's calculations there occurs another very essential oversight. To prove this I will follow his own developments further, merely remarking that wherever in the sequel the fraction $\frac{r}{\alpha}$ occurs, the somewhat altered value $\frac{r}{\alpha} - \frac{r}{\alpha^2}\frac{dr}{dt}$ should be inserted.

In order to deduce from the potential of the individual electrical masses ϵ and ϵ' the potential of the entire electrical currents S and S', he considers the summations executed in reference to all the quantities of electricity in the two conductors. Hence from this the potential of S upon S' would be

$$-\Sigma\Sigma\epsilon\epsilon'\mathrm{F}\left(t - \frac{r}{\alpha},\ t\right).$$

But, for the transformations which he intended, it was necessary to have the time in the formula differently from the manner in which it occurs; and he therefore considers the potential not merely at the time t, but during an interval of time from 0 to t; and the expression thus formed he considers to be the potential of the forces exerted from the time 0 to the time t. If this potential be denoted by P, we get

$$\mathrm{P} = -\int_0^t \Sigma\Sigma\epsilon\epsilon'\mathrm{F}\left(\tau - \frac{r}{\alpha},\ \tau\right)d\tau. \quad . \quad . \quad . \quad (6)$$

To calculate this magnitude P, it would have been simplest to develope the function occurring in the expression according to $\frac{r}{\alpha}$. Then, neglecting higher members, there would have been obtained

$$\mathrm{P} = -\int_0^t \Sigma\Sigma\ \epsilon\epsilon'\left[\mathrm{F}(\tau,\ \tau) - \frac{r}{\alpha}\mathrm{F}'\tau,\ \tau) + \frac{r^2}{2\alpha^2}\ \mathrm{F}''(\tau,\ \tau)\right]d\tau,$$

or, otherwise written,

$$\mathrm{P} = -\int_0^t \Sigma\Sigma\ \epsilon\epsilon'\left[\frac{1}{r} - \frac{r}{\alpha}\frac{d\left(\frac{1}{r}\right)}{d\tau} + \frac{r^2}{2\alpha^2}\frac{d^2\left(\frac{1}{r}\right)}{d\tau^2}\right]d\tau.$$

This expression, in which occur only differential coefficients which refer to the motion of ϵ, becomes equal to zero if the summation

be effected of the quantities of electricity ϵ' in the conductor of the current S', which are so distributed that there is everywhere the same quantity of positive and of negative electricity.   It follows thence that the expression (6), proposed by Riemann for the potential, is not fitted to explain the action of two currents upon each other.

An expression which is to fulfil this object must necessarily contain a member in which a differentiation in respect to the motion of ϵ as well as one in reference to the motion of ϵ' occurs—a member, that is, which contains either the product $\dfrac{d\left(\dfrac{1}{r}\right)}{d\tau}\dfrac{d'\dfrac{1}{r}}{d\tau}$, or the differential coefficients of the second order,

$$\frac{dd'\left(\dfrac{1}{r}\right)}{d\tau\,d\tau}.$$

To introduce such a member into his expression, Riemann effects a peculiar operation.

He first writes

$$\mathrm{F}\left(\tau-\frac{r}{\alpha},\,\tau\right)=\mathrm{F}(\tau,\,\tau)-\int_0^{\frac{r}{\alpha}}\mathrm{F}'(\tau-\sigma,\,\tau)d\sigma.$$

If we introduce the difference which stands here on the right-hand side into the expression (6), it divides into two members, the first of which, containing $\mathrm{F}(\tau,\,\tau)$, becomes zero by summing all the quantities of electricity ϵ and ϵ', owing to the uniform distribution of positive and negative electricity.   Hence there remains

$$\mathrm{P}=\int_0^t d\tau\,\Sigma\Sigma\epsilon\epsilon'\int_0^{\frac{r}{\alpha}}\mathrm{F}'(\tau-\sigma,\,\tau)d\sigma.\ \ .\ \ .\ \ .\ \ .\ (7)$$

Riemann now changes the order of the integrations by writing

$$\mathrm{P}=\Sigma\Sigma\epsilon\epsilon'\int_0^{\frac{r}{\alpha}}d\sigma\int_0^t d\tau\,\mathrm{F}'(\tau-\sigma,\,\tau).\ \ \ .\ \ .\ \ .\ (8)$$

In this he puts $\tau+\sigma$ for τ, by which he gets

$$\mathrm{P}=\Sigma\Sigma\epsilon\epsilon'\int_0^{\frac{r}{\alpha}}d\sigma\int_{-\sigma}^{t-\sigma}d\tau\,\mathrm{F}'(\tau,\,\tau+\sigma).$$

If in this expression the limits $-\sigma$ and $\tau-\sigma$ of the second integral are to be replaced by the limits 0 and t, to the integral thus changed another must be added which goes from $-\sigma$ to 0, and another from $t-\sigma$ to t subtracted from it.   But Riemann shows subsequently that the members resulting from these two

integrals are infinitely small ; hence instead of the preceding we may write

$$P = \Sigma\Sigma\epsilon\epsilon' \int_0^{\frac{r}{\alpha}} d\sigma \int_0^t d\tau F'(\tau, \tau + \sigma). \quad . \quad . \quad . \quad (9)$$

This expression is distinguished from that given under (8) by the altered form of the function ; and after this change has been effected, Riemann again alters the order of the integrations and thereby obtains

$$P = \int_0^t d\tau \Sigma\Sigma\epsilon\epsilon' \int_0^{\frac{r}{\alpha}} d\sigma F'(\tau, \tau + \sigma). \quad . \quad . \quad (10)$$

From this point the further calculation is very simple. If the function under the second integral sign is developed according to σ, we get, neglecting the higher members,

$$P = \int_0^t d\tau \Sigma\Sigma\epsilon\epsilon' \int_0^{\frac{r}{\alpha}} d\sigma [F'(\tau, \tau) + \sigma F'_1(\tau, \tau)],$$

or, by performing the second integration,

$$P = \int_0^t d\tau \Sigma\Sigma\epsilon\epsilon' \left[\frac{r}{\alpha} F'(\tau, \tau) + \frac{r^2}{2\alpha^2} F'_1(\tau, \tau) \right].$$

This expression resolves into two members, of which that containing $F'(\tau, \tau)$ disappears when the summation is performed with respect to ϵ', and there remains

$$P = \int_0^t d\tau \Sigma\Sigma\epsilon\epsilon' \frac{r^2}{2\alpha^2} F'_1(\tau, \tau) ; \quad . \quad . \quad . \quad (11)$$

or, differently written,

$$P = \int_0^t \Sigma\Sigma\epsilon\epsilon' \frac{r^2}{2\alpha^2} \frac{\left(dd'\left(\frac{1}{r}\right)\right)}{d\tau\,d\tau} d\tau. \quad . \quad . \quad . \quad (12)$$

This is the expression deduced by Riemann, which represents the electrodynamic action of the two currents upon each other.

That which I consider to be incorrect in this analysis is the manner in which Riemann exchanges the integrations in order to pass from equation (7) to equation (8), and afterwards from (9) to (10). This exchange would only be admissible in case the magnitude $\frac{r}{\alpha}$, which forms the upper limit of one integral, were independent of the time τ, according to which the other integration is to take place. This, however, is not the case, but, if the quantities of electricity ϵ and ϵ' move, their distance r is variable with the time. Hence the equations which are obtained by

these changes cannot be regarded as correct consequences of the earlier equations.

I believe that Riemann subsequently convinced himself of this error, and that this was the reason why he withdrew his paper. Yet its publication has not been without use for science; for though it has not yet solved the question as to the origin of electrodynamic forces, it has contributed to moot this question afresh, and to give to it a heightened interest; so that at present several physicists and mathematicians have had their attention directed to it, and a solution may perhaps thereby be attained.

Of the above-mentioned three papers, there remains to be considered that of Betti, which appeared in the *Nuovo Cimento*, vol. xxvii.

Betti figures to himself as follows the two constant currents whose potential on each other he wishes to determine. He supposes the closed curves traversed by the currents to consist of elements which are periodically polarized, and therefore act upon each other as if they were magnetic elements whose axes are parallel to the tangents of the curves. He assumes that the periods of the changes are the same in both currents, so that they can only be distinguished by their phases. He adds to this idea the assumption that, for the propagation of the action of an element of one current to an element of another, a certain time is necessary which he calls $\frac{r}{c}$, r being the distance of the two elements from each other. He thus obtains for the potential of the two currents upon each other the same expression as that known in electrodynamics.

It might seem that the assumptions made had been fully confirmed, and that thus the question as to the nature of the electrodynamic forces had been solved in a different manner from that attempted by Riemann and Neumann. Yet it may be shown that here, apart from an improbability inherent in the mode of representation, upon which I will not enter, there is an oversight in the mathematical development which is of essential influence upon the result.

To show this, it is not necessary to follow the entire investigation, but it is sufficient to consider that part in which it occurs.

In order to express mathematically the periodical changes of the polarized elements, Betti introduces a function of the time $\phi(t)$ which has the property of regularly altering its value in very short intervals. The duration of the periods he designates by p. Of this function the value is to be determined which it assumes if t is replaced by $t + \sigma - \frac{r}{c}$, in which σ may be any magnitude between 0 and p, and $\frac{r}{c}$ the above-mentioned time which

is necessary for the propagation of the action. To determine this value, Betti developes the function in powers of $\sigma - \dfrac{r}{c}$, and limits himself to the first powers by putting

$$\phi\left(t + \sigma - \frac{r}{c}\right) = \phi(t) + \left(\sigma - \frac{r}{c}\right)\phi'(t) + \left(\sigma - \frac{r}{c}\right)^2 \frac{\phi''(t)}{2}.$$

The limitation to the first two powers he justifies on the ground that σ and $\dfrac{r}{c}$ are small magnitudes. But, according to his own assumption, the duration of a period of the function represented by $\phi(t)$ is also very small, and in the course of his deduction there is even the following passage:—" Now let the duration p of a period be very small as compared with the time in which the electrical action propagates itself through the unit of length, so that σ (the magnitude lying between 0 and p) may be neglected in comparison with $\dfrac{r}{c}$."

If the function $\phi(t)$ has so short a period, and its value therefore changes so rapidly as is here presupposed, it must have very great differential coefficients. If such a function is to be developed in reference to a magnitude which contains $\dfrac{r}{c}$, and in comparison with which the duration of a period is very small, it is impossible to neglect in this development all higher powers than the second. To see this, since Betti has named no special conditions as regards the nature of the periodical functions, it is sufficient if we consider any given function whose period has the duration p. Let it be the following:

$$\sin \frac{2\pi}{p} t.$$

Putting in this $t + P$ in the place of t, and developing with respect to powers of P, we have

$$\sin \frac{2\pi}{p}(t + P) = \sin \frac{2\pi}{p} t + P \frac{2\pi}{p} \cos \frac{2\pi}{p} t - \frac{P^2}{1 \cdot 2}\left(\frac{2\pi}{p}\right)^2 \sin \frac{2\pi}{p} t$$
$$- \frac{P^3}{1 \cdot 2 \cdot 3}\left(\frac{2\pi}{p}\right)^3 \cos \frac{2\pi}{p} t + \&c.$$

In this series we see at once that when the magnitude p in the denominator is small as compared with the magnitude P in the numerators, we must not think of restricting ourselves to the first three members.

Since in Betti's paper the entire further calculation depends upon the development of that series in which all members higher

than the second power are omitted, members are wanting in the result which should not be omitted; and therefore this result cannot, until otherwise proved, lay claim to any validity.

I think I have thus shown that all the investigations cited at the outset of my paper, however cleverly they otherwise treat the matter, contain certain faults which make it impossible to admit that the results are correctly deduced, and that the solution of the problem of referring electrodynamic forces to known electrostatic forces has not been attained in these investigations.

Würzburg, October 20, 1868.

LXIII. *On some Spectrum Observations of Comets.* By WILLIAM HUGGINS, *F.R.S.**

THERE is another class of heavenly bodies distinct from the objects we have considered—the comets.   Of the nature of the phenomena presented by these strange masses of light of constantly changing form we possess but little certain information.   It is doubtless to spectrum analysis that we must look for any important increase of our positive knowledge of the constitution of those bodies, and of the true nature of the remarkable changes which take place in them under the action of the solar rays.

A not unimportant earnest of the more complete information which the prism will doubtless exact from the next brilliant comet has been already gained by the examination of five faint comets. One of these was examined by Donati in 1864†.   Two of these, which were excessively faint, I observed in 1866‡ and 1867§. The others, which were in a small degree brighter, I examined in the summer of last year.

These observations showed that the greater part of the light from the heads of comets is very different from solar light, and therefore cannot be the sun's light sent to us by *ordinary* reflection from the cometary matter.

In the case of the very faint comets of 1866 and 1867, I was not able to determine more than that the light of the brightest part of the coma consisted for the most part of green rays of the refrangibility between *b* and F of the solar spectrum.   Further, as far as such very faint objects would permit of observation, I suspected that the margin of the coma and the tail gave a conti-

* From the Rede Lecture, delivered at Cambridge, May 18, 1869. Communicated by the Author.

† *Astronomische Nachrichten*, No. 1488 ; and Monthly Notices of the Royal Astronomical Society, vol. xxv. p. 490.

‡ Proceedings of the Royal Society, vol. xv. p. 5.

§ Monthly Notices of the Royal Astronomical Society, vol. xxvii. p. 288.

nuous spectrum, and was probably solar light sent to us by ordi-
nary reflection.

Previous telescopic observations had shown that in several
bright comets the light of the head differed from solar light in
having a decided blue tint*. In this part of the comet the po-
lariscope has generally given but feeble indications of the pre-
sence of reflected light. These characters are in accordance
with the spectroscopic examination of the light of this part of
comets.

I will now describe the more complete analysis of the blue
cometary light which the brighter comets of last summer enabled
me to make.

One of these was Brorsen's comet at its return in 1868, the
other a comet discovered by Winnecke†.

Winnecke's comet (Comet II. 1868) presented in the telescope
a nearly circular coma surrounding a bright nebulous spot, where
probably a true nucleus existed. The faint nebulosity from the
margin of the coma could be traced for more than a degree, and
formed a tail which was sharply defined on the following edge,
but faded away so gradually on the opposite side that no limit
could be perceived. In the spectroscopic observations the slit
was placed across a diameter of the head.

The spectrum of this comet, and also that of Brorsen's comet,
consisted of three bright bands in similar but not identical parts
of the spectrum. The circumstance that the bands were nar-
rower than those of the other comet might be due to the smaller
intensity of the light of this comet, in consequence of which the
bands could not be traced so far in the instrument. If this pos-
sible explanation of the difference in breadth of the bands be
admitted, there will remain the difference of refrangibility in
the strongly marked beginning of the middle bands of the two

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* Sir William Herschel described the head of the comet of 1811 to be
of a greenish or bluish-green colour, while the central point appeared to be
of a pale ruddy tint. The representations of Halley's comet at its appear-
ance in 1835 by the elder Struve are coloured bluish green, and the nucleus
on October 9 is coloured reddish yellow. He describes the nucleus on that
day thus :—" Der Kern zeigte sich wie eine kleine, etwas ins gelbliche
spielende, glühende Kohle von länglicher Form " (*Beobachtungen des Hal-
ley'schen Cometen*, p. 41). Dr. Winnecke describes similar colours in the
bright comet of 1862 :—" Die Farbe des Strahls erscheint mir gelbröthlich,
die des umgebenden Nebels (vielleicht aus Contrast) mattbläulich. . . . .
Die Farbe der Ausströmung erscheint mir gelblich ; die Coma hat bläu-
liches Licht" (*Mémoires de l'Académie Impériale des Sciences de St. Péters-
bourg*, vol. vii. No. 7).

† For a more detailed account of the observations of these comets and
diagrams of their spectra, the reader is referred to papers by the lecturer in
the Proceedings of the Royal Society, vol. xvi. p. 386 ; and Phil. Trans.
1868, p. 555.

comets—a difference sufficiently great to support the conclusion that the conditions in the two comets by which the light was furnished were not identical.   The first band occurs about half-way from D to E of the solar spectrum ; the second band begins about *b* and extends nearly to F.   The third band presents itself between F and G.

The three broad cometary bands differ greatly in refrangibility and in character from the sharply defined narrow lines of the nebulæ, and appear to show a constitution distinct from that of the nebulæ.

The morning after I had made the observations of Winnecke's comet, I was much interested to find that the comet's spectrum appeared to be identical with one of a series of the spectra of carbon, as obtained from the decomposition by the induction-spark of several compounds of carbon which I had prepared some years before.

The modification of the spectrum of carbon, which appeared identical with the cometary spectrum, presented itself when the spark was taken in olefiant gas and some other compounds of carbon, and differs from the apparently more perfect spectrum which is obtained by the decomposition of cyanogen and olive-oil, &c., in the one circumstance alone that the three bands in the bright parts of the spectrum are not resolved into distinct narrow lines, but the light in each band becomes gradually fainter without suffering any break in its continuity.

I believe we have a right to consider this peculiar spectrum, containing the three bright bands, to be the spectrum of the vapour of carbon and not that of any stable hydrocarbon, for the reason that I obtained the same spectrum when I used olive-oil, the vapour of Persian naphtha in hydrogen, and other hydrocarbons, as when I employed cyanogen.   In one case the spectrum was accompanied by the lines of hydrogen, in the other by the known complex spectrum of nitrogen.   A spectrum essentially the same, though less complete, was obtained, together with the known lines of oxygen, when carbonic acid and carbonic oxide were employed.

In the evening of the same day I compared the spectrum of the comet directly with the spectrum of olefiant gas, the two spectra being juxtaposed in the instrument.   Careful comparisons made on that evening, when my friend Dr. W. Allen Miller observed with me, and on two subsequent nights, showed that in every particular of refrangibility and of relative intensity, the spectrum of the comet was similar to that of carbon.

The obvious and apparently well-founded conclusion from these observations would be that the cometary matter from which this light comes consists of the luminous vapour of carbon.

It must not be overlooked that on this supposition a formidable difficulty presents itself in the insufficiency of the degree of solar heat to which this comet was subjected for the conversion of carbon into vapour. We cannot, for several reasons, suppose that the solar heat was supplemented by the heat of chemical action which it called forth. The suggestion, however, that carbon may possibly exist in an allotropic state in which it may be much less fixed, and so capable of passing into vapour at a comparatively low temperature, is not perhaps inconsistent with our positive knowledge of other elements. Even if this suggestion were true to fact, a difficulty would remain ; for, so far as we know, vapour in the non-luminous state, though it would not transmit light of the refrangibilities which it would emit when heated, would not give them back by reflection, so as to reflect light similar to that which is peculiar to it in the luminous condition *.

It may be well to consider for a moment the principal phenomena which usually present themselves when a comet approaches the sun.

(1) Under the influence of the solar force the nucleus throws out luminous jets which frequently assume the form of luminous envelopes about it †.

(2) The jets or envelopes rise, in the first instance, towards the sun.

(3) The envelopes are frequently separated from the head, and from each other, by invisible spaces.

(4) At the boundary of the head the envelopes behave as if they had become subject to an intense force of repulsion from the sun.

(5) The matter of the envelopes appears to be driven from the sun on all sides of the head, and in this way to form a hollow conical tail.

Though the feeble light of the comets hitherto subjected to spectrum analysis has permitted them to be but imperfectly investigated, we have learned that the matter which emanates from the nucleus, and is distinguished by a blue tint, gives a light which the prism shows to be identical with that emitted by the vapour of carbon. It is certain, therefore, that the light which has the blue colour is not due to reflection from a cloud of which the particles are too small to reflect the longer waves of the less refrangible colours.

* It might be possible that a spectrum of bright bands would be given by a gas in a fluorescent state ; but the circumstance of the coincidence of the cometary spectrum with that of carbon would remain unexplained.

† Of these phenomena see a graphical account by Sir John Herschel, ' Familiar Lectures on Scientific Subjects,' p. 115 ; also the " Account of the Great Comet of 1858," by G. P. Bond, which forms vol. iii. of the ' Annals of the Observatory of Harvard College.'

The invisible spaces between the envelopes may possibly correspond to a condition of the vapour too cool to emit light, and yet not condensed so as to reflect light.

The exterior parts of the coma and the tail, which have been found to be polarized in a plane, showing the light to come from the sun, may be supposed to consist of the vapour of the nucleus condensed into widely scattered particles of great minuteness.

The remarkable phenomenon of the great rapidity with which the tail is seen to extend itself to enormous distances in a direction from the sun remains unexplained. It may be suggested that the instant at which the matter appears to come under the influence of repulsion from the sun, may be that at which the vapour is condensed into discrete particles, and be in some way connected therewith*.

Many years since Benedict Prevôt† suggested the following hypothesis. The head of the comet by the sun's heat is converted into vapour which is invisible, and expands to a great distance from the head in all directions. Behind the head, where the vapour is sheltered from the sun's heat, it is condensed into cloud, which reflects light and appears as the tail. This cloud passes back into invisible vapour as, by the comet's motion, it becomes exposed again to the solar beams.

This theory is obviously inconsistent with the observed appearances and forms of the tails, and especially with the rays which are frequently projected in a direction different from that of the tail, with the absence of tail immediately behind the head, and with the different degrees of brightness of the sides of the tail.

I should not refer to this almost-forgotten hypothesis, but for the circumstance that the same theory essentially forms part of the recent ingenious speculations of Professor Tyndall on the nature of comets, with the difference, however, that clouds formed in the cool shade of the comet's head are supposed by him to be due to chemical action which can then take place, and not, as in Prevôt's theory, to a lower temperature alone.

For further positive knowledge of the nature of cometary phenomena we must doubtless wait until the searching method of analysis by the prism can be applied to the series of changes presented by a brilliant comet.

* Phil. Trans. 1868, p. 563.
† Arago, ' Popular Astronomy,' translated by Smyth and Grant, vol. i. p. 623.

LXIV. *On Statical and Dynamical Ideas in Chemistry.*—Part I. *Acid, Alkali, Salt, and Base.* By Edmund J. Mills, *D.Sc., F.C.S.**

1. THE history of chemistry exhibits, in one respect, a re- markable parallel to the history of philosophy ; for no other sciences have transmitted to the present epoch so many unresolved and kindred controversies. The question of the One and the Many, for example, is allied to that in which the unity of matter is challenged by the variety of elements ; discussions as to Absolute and Infinite resemble those which have reference to the divisibility of matter ; Kosmos reappears in the law of Definite Proportions ; and the contest about general principles was an echo of the battles of realism. Of the attempts to de- cide satisfactorily upon philosophical disputes, Ferrier considered his own † the best and purest ; and it is a masterpiece among its kind : but I believe that no chemist has ever proposed an enter- prise like that of the philosopher ‡. Nevertheless there are pro- bably but few chemists who, amid the assault and confusion of modern theories, would decline a criterion by which to estimate the multitudinous claims so pertinaciously presented for their allowance. Such a criterion seems attainable by the following considerations.

Adopting the primitive procedure of reason itself (which con- sists in comparing one object with another), inductive science has uniformly selected the analogical method as the basis of her progress. In this manner, the notion of *comparability* has con- ferred upon scientific reasoning a peculiar character, and made it, for the most part, a process of convergence. The history of the different sciences may, indeed, be compared to a group of con- verging series having a common limit—that limit being the law, for ever to be desired, from which all phenomena may be de- duced. Such is the point towards which analogy, by compari- son, and by abstracting differences, is ceaselessly making an ad- vance. If this be admitted, a criterion of scientific progress becomes possible ; and that criterion is the most general idea existing at a given time as a factor in every branch of science. An idea is, for this purpose, preferable to a law, because ideas always determine the form of contemporary laws. Now it will probably be conceded that the criterion at present required is at-

* Communicated by the Author.

† Institutes of Metaphysic, first edition, p. 63.

‡ Freind's work, entitled "Chymical Lectures : In which almost all the Operations of Chymistry are Reduced to their True Principles and the Laws of Nature" (London, 1737), must not be supposed to have been written with the intention above indicated.

tained in the idea of *motion*, such motion being understood of itself
—that is (in ordinary language), without reference to anything
moved.   This criterion will sufficiently enable us to decide upon
the relative value of most of the current theories, of chemistry
especially.   I purpose in this paper to apply it to the contro-
versy about *acid, alkali, base,* and *salt* ; but the following histo-
rical digression is a necessary preface to the argument*.

2. The word *acid* was probably used at first only in a concrete
sense, signifying vinegar.   The solvent properties of what we
now call acids were noticed by Geber.   Tachenius (an iatro-
chemist) defines them as forming salts with alkalies, to which
they have a certain oppositeness or antagonism.   Boyle and
others noticed that they reverse the colorific effects of alkalies.
Freind testifies to the general acceptance of the Tachenian doc-
trine during the first third of last century, and strongly protests
against it ; the corrosive, colorific, and fermentative properties
are, he states, often shared by acids and alkalies alike, " and
what in respect to one body is named *alkali*. is, if compared
with some other, by the very same writers call'd an *acid*.   So
that in vain we endeavour to fix the boundaries which separate
each kind."

Various opinions have been proffered as to the source of the
acid properties.   Becher and Stahl ascribed them to the pre-
sence of the universal acid, Sylvius to a fiery matter, Meyer
to an " acidum pingue "—the last two being supposed to be
common also to alkalies.   Lemery assured himself of the sharp-
ness of the constituent particles of acids ; Mayow, Scheele, and
Lavoisier ascribed acidity to oxygen.   Davy, on the other hand,
first considered hydrogen as the acidifying principle ; but he
afterwards discarded the notion of a principle, and held, if I may
so illustrate his view, that the acidity of any substance is a kind
of resultant whose direction is hydrogen.   The doctrine that " an
acid is a salt of hydrogen " may be assigned to Dulong and
Gerhardt (the former especially).

A question of nomenclature has arisen recently with reference
to this subject.   Professor Williamson maintains† that the words
ACID and BASE " were introduced to describe bodies of oppo-
site properties which are more or less completely lost in the salt
or compound of acid and base " (p. 423), or " belong to the
idea of compounds of fundamentally opposite properties which
unite to form one or more molecules of a comparatively neutral

* The following historical details have been purposely compressed as
much as possible.   Authority for many of them will be found in Kopp's
Geschichte der Chemie, or in Watts's ' Dictionary of Chemistry ; ' but they
are partly supported by original research.

† Chem. Soc. Journ. vol. xvii.

compound" (p. 424); and he proposes to revert to this usage, confining the word acid to what Gerhardt called "anhydrides." Certain bodies which resemble hydric, ferrous, and ferric chlorides are termed "normal salts" and "acid in their properties." Dr. Williamson also remarks that "chemists might just as well limit the word acid to the salts of lead, calling the acids themselves "anplumbates," as say with Gerhardt, that hydrogensalts are the only acids, and that the real acids are not acids, but only anhydrides;" and he adds that "the hydrogen-salts cannot with any consistency be called acids." Professor Foster* defends the common application of the word "acid." He shows that, for some little time prior to the definition of Gerhardt, the term included both hydric salts and hydrides; but that, as a matter of fact, "in perhaps ninety-nine cases out of a hundred, when an acid was spoken of as taking part in, or resulting from, a reaction, it was a hydrogen-salt, and not an anhydrous acid, that was meant." On the whole, however, he is disposed to think that the word "indicates a distinction to which we now know that no real difference corresponds," and advocates terming acids "hydric salts," and the anhydrides, what they really are, merely "oxides."

Alkali originally signified crude potash. Van Helmont, Sylvius, Lemery, and Boerhaave applied it in a general sense to bodies which effervesce with acids; but Boyle recognized as alkalies certain substances which do not act in that manner. The iatro-chemists noticed that they neutralize the effects of acids. Kunkel limited the name to what also might, as he believed, be transformed into acids; ready union with acids was the characteristic advocated by Stahl. When alkalies came to be divided into CAUSTIC and EFFERVESCENT, it was assumed that the action of lime in transforming the latter into the former kind consisted in a transference of one of its own constituents to the effervescent alkali. This constituent was supposed by Lemery and others to consist of igneous particles; Kunkel assigned to it both weight and acid properties. Stahl supposed that alkalies contain a minimum of the primitive acid, of which common acids and neutral and alkaline salts are also partakers. Black, by an admirable series of inductive experiments carried out quantitatively, showed that FIXED AIR is the cause of loss or diminution of causticity. Meyer controverted this conclusion; and a train of reasoning, which we may still admire for its subtlety and system, led him to refer causticity to the *acidum pingue* —an igneous matter of acid nature. So ably did Meyer support this theory, that two of the most eminent contemporary chemists gave it special refutation.

* Phil. Mag. S. 4. vol. xxi. p. 262.

The term *salt* was likewise concrete in its primitive significa-
tion, having been from time immemorial applied to culinary salt.
Aristotle denoted by it the evaporated lixivium of wood-ash;
Dioscorides and Pliny seem to have called crude soda by this
same name, the latter naturalist using it generally for substances
which could be recovered from water by evaporation. Such per-
haps was the practice of Geber and certain alchemists of the
west.   Basil Valentine classified the vitriols apart as metallic
salts; and from his time the word SALT had, as one of its mean-
ings, that of a constituent indestructible by ignition.   This, ac-
cording to Paracelsus, may be found in all bodies; moreover
alum and the vitriols are salts.   Other chemists of the sixteenth
century understood by salts substances which in taste and solu-
bility resemble common salt.   Palissy included sugar in the
list.   In the seventeenth century Lemery defined a neutral salt
(*sel salé*) as an alkali charged with acid; Van Helmont observed
that salts are composed of two opposite constituents; Tachenius
stated that all salts are decomposable into acid and alkali.   Ac-
cording to Becher, a salt contains elementary earth and water;
according to Stahl, acids, alkalies, and salts are transmutable
inter se, and consist of the same ingredients.   Newton called
water a salt.   In the next century Boerhaave defined salts as
soluble in water, fusible, and sapid; the vitriols he excepted, as
being semimetals.   Bergmann and Kirwan depended especially
upon taste and solubility in a certain proportion of water.

Rouelle (whose example was followed by Trommsdorf and La-
voisier) regarded a salt as a compound of an acid and a base.
But, as analytical knowledge advanced, it was found that the
definition required to be amended in order to include sodic chlo-
ride and its analogues; and accordingly we find Berzelius, in the
year 1825, arranging a new and matured classification of salts, and
specifying *electrochemical indifference* as the appropriate idea to be
connected with them.   Gerhardt defined a salt as a binomial
("*corps binome*"), prone to double decomposition; and Griffin
understood by it " a compound of two radicals."

The idea represented by the term *Base* is much older than the
word.   Thus, in Lemery's conception of a salt, it is the alkali
which comes first, and is " cloyed or filled with acid."   Stahl
also, referring to the substance which, in common salt, he sup-
posed to be united with hydric chloride, calls it " materia illa
quæ sali *corpus* præbet."   Duhamel, Rouelle, and others in the
last century used the name *base* to express " that which gives a
concrete or solid form" to a salt.   In this century it has been
chiefly used to signify an oxidized body, having properties com-
plementary to an acid; at present it stands in a generic relation
to the word alkali.

In the year 1809 Avogadro published a most remarkable memoir, entitled " Idées sur l'Acidité et l'Alkalinité"*, which appears to have hitherto almost escaped the attention of chemists; yet it embraces, with marvellous skill and simplicity, the whole of this interesting question. After pointing out the well-known difficulties which stood in the way of the oxygen theory of acids, he shows that the idea of acidity involves two factors—namely, an *antagonist force* (reciprocated by alkalies), and a great tendency to unite with bodies in general. The latter seems to depend chiefly on a certain state of aggregation, which may either allow full play to the former or almost entirely prevent its action; hence this state of aggregation is not a cause, but the condition of acidity. " Such being the case, all the phenomena are easily explained if we consider acid and alkaline antagonism as purely relative properties, only becoming somewhat absolute when referred to a middle term fixed arbitrarily in the scale of acidity and alkalinity; so that the same substance A which, with reference to B, has the acid antagonism, may possess the alkaline antagonism with reference to a third substance, C; whence what we term absolutely acids and alkalies are merely bodies which have the acid or alkaline antagonism in respect of certain others whose position in the scale is approximately indicated by certain properties, such as inability to affect vegetable blues, though their state of aggregation be suitable for the purpose." The degree of acidity or alkalinity of a compound depends on the degree of those properties in its constituents. Thus considered, " *of two substances in the act of combination, one always plays the part of acid and the other of alkali; and it is this antagonism which constitutes tendency to combination, or* AFFINITY *properly so called.*"

Bodies might thus be arranged in a series, the position of each marking its true affinity to any successor. Oxygen and sulphur would probably come first, and the neutral salts in the middle; while hydrogen, carbon, and the like would occupy the other extremity. The measure of chemical antagonism is electric heterogeneousness; its appropriate name is *oxygenicity*†. Oxygen is the most oxygenic of bodies; and a substance is evidently more oxygenic the less it is oxidizable. Avogadro concludes by recommending (as Professor Foster did subsequently) the disuse of the word acid, on account of its representing, as commonly received, a merely accidental property. It will suffice, he thinks, to employ the nomenclature of oxides (e. g. *higher* or

* *Journ. de Phys.* p. 142 *et seq.*

† *Electricity* and *electric* are obviously the pattern words for *oxygenicity* and *oxygenic*. Avogadro seems to have proposed the latter on account of their reference to chemistry.

lower oxide) or salts as a case may require. These views, it may be added, were fully retained by their author when (two years later) he wrote his celebrated ' Essai.'

3. The preceding historical survey is sufficient to show that some of the inadvertencies of chemical thinking have been for hundreds of years precisely the same as those of ordinary thinking, and consequently abundantly exemplified in our daily life. In our habitual (and for the most part uncritical) mood the mind is in a fixed attitude, and its object is a sedentary image ; it resembles a mirror in an unoccupied apartment.   If not purely receptive, it either postpones inquiry, or soon bounds that inquiry with the kind of limit that is known as belief.   In this manner statical ideas arise.   On the other hand, a mind in which every event is criticised on its occurrence, by the sum total of its predecessors, can only evolve dynamical ideas.   *Doubt* is the popular representation of such a condition.   Now neither of these states is ever exclusively realized ; but history and common experience show that, of the two, the former accurately describes the greater part of our intellectual existence, the latter designates its occasional or unsystematic life.   Nevertheless it is *this* which it is advantageous to make uniform ; the abandonment of statical ideas necessarily follows from the criterion of motion.

Acid, alkali, and *salt* have had, it appears, very varying but kindred significations. They all at first were names for one concrete substance respectively.   They were all at times generically used for bodies whose properties were explained by universals ; of which the " universal acid " of Stahl, the " acidum pingue " of Meyer, and the " igneous particles " of Lemery are examples, and from which the " acidifying principle " of Lavoisier only differed by being isolated. These universals were supposed really to exist in bodies and to constitute part of them.   The finest conception of this kind was the primitive acid of Stahl, supposed to be common to acids, alkalies, and salts ; few men have so closely approached a dynamical theory as he did and yet failed to reach it.

Tachenius was undoubtedly the first to give a clear statement of the dualistic doctrine respecting salts.   But if, as Freind asserted, his doctrine led to this result, namely, that acids and alkalies have only a relative existence, Tachenius deserves a nobler monument than obscurity ; for he must be credited with one of the first dynamical theories in chemistry.   The transition from this to certain later views is easy.   Davy's experiments in electrolysis suggested to Avogrado the idea of a chemical force polar at the moment of action, and presiding not only over the union of acid with alkali, but over every chemical change. Berzelius afterwards announced that salts are electrochemically in-

different; and Laurent, Graham, and Brodie have successively contributed (each from his own point of view) to the furtherance of the idea of polarity.

One result of these dynamical doctrines has been, that we have now current among us a tolerably pure idea of a salt—as a substance prone to double decomposition. The term *base* (now including the ancient *alkali*) is admitted on all sides to involve an idea complementary to that of *acid*. This last word is still under discussion. According to the prevailing definition, an acid is a salt of hydrogen; according to Professor Williamson it is what was called by Gerhardt an anhydride. Neither of these is in accordance with the criterion; for they both agree in calling acid *something particular*. Now if we really mean to assign to acids properties which are opposite to those of bases, it follows that hydric sulphate and hydric acetate are, with sulphuric teroxide and diacetylic oxide, equally acids. Thus, in the equations

$$KHO + H^2 SO^4 \qquad = KH SO^4 + H^2 O,$$
$$KHO + SO^3 \qquad = KH SO^4,$$
$$KHO + C^2 H^4 O^2 \quad = KC^2 H^3 O^2 + H^2 O,$$
$$KHO + (C^2 H^3 O)^2 O = KC^2 H^3 O^2 + HC^2 H^3 O^2,$$
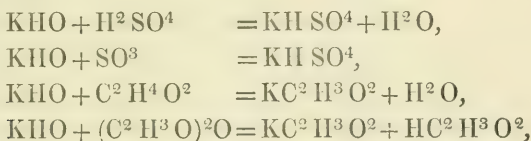
the four bodies indicated are alike antagonistic to hydropotassic oxide. Again, the reactions

$$As^2 O^3 + 6 HCl = 3 H^2 O + 2 As Cl^3$$

and

$$As^2 O^3 + 2 KHO = H^2 O + K^2 As O^4$$
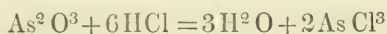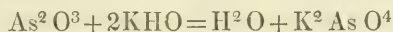
prove either (1) that arsenious oxide is both a base and an acid, or (2) that hydric chloride and hydropotassic oxide are both acids or both bases. Such are the contradictions which must necessarily ensue as long as we attempt to carry out in practice a statical definition on a dynamical understanding.

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The value of the idea of motion as a criterion in chemical theory has, I trust, been indicated in the preceding paper. As a criterion, however, it really has a far wider usefulness than I have here assigned it; for it throws light into every branch of knowledge. Several advanced problems (more especially in ethics) yield remarkable results on its application, and I intend to take an early opportunity of again referring to its efficacy.

LXV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Jan 21, 1869.—John Peter Gassiot, Esq., Vice-President, in the Chair.

THE following communication was read :—

"On the Thermal Resistance of Liquids." By Frederick Guthrie.

The memoir of which the following is an abstract gives an account of some experiments made by the author with the object of determining the laws according to which heat travels by conduction through liquids.

After pointing out the importance of the subject, and briefly recapitulating the methods previously used and the results obtained by other experimenters, the " Diathermometer " is described.

This instrument, which may be employed for the examination of the thermal resistance or conducting power of solids as well as liquids, has the following form. A hollow brass cone, having a platinum base, is screwed with its apex downwards into a tripod stand which rests upon adjusting screws. The apex of the cone is tubular, and carries a cork, through which passes a vertical glass tube graduated and dipping into water. The level of the water in the tube is nearly as high as the apex of the cone. By means of a micrometer screw, a second cone, exactly similar and equal to the first, having its apex upwards, may be brought to any required distance from the lower cone. The brass cones and their platinum faces are highly polished, and the latter are cleaned by washing successively with hot nitric acid, caustic soda, alcohol, and water. The upper surface of the lower cone is brought into an exactly horizontal position, and the upper cone is lowered to any required distance from it. There is thus formed between the platinum faces a cylindrical interval of known height or thickness, and diameter, and having its opposite faces parallel and horizontal. This wall-less chamber receives the liquid whose thermal resistance has to be measured. A liquid, introduced by means of a strangulated pipette of known capacity (equal, say, to the interstitial space when the cone-faces are 1 millim. apart) between the cones, remains there by means of its adhesion and cohesion. A description is given of the method used to get a constant current of water of uniform and known temperature to pass through the upper cone. When such a current passes, the platinum face of the upper cone becomes heated; it communicates its heat to the liquid in contact with it. The heat passes downwards through the liquid, heats the upper surface of the lower cone, expands the air therein, and depresses the level of the water in the tube attached to the lower cone.

A description is given of the most prominent sources of error of this instrument, and the means which were employed to eliminate them. It is concluded, from direct experiments (1st by measuring

the time required for the production of the first heat-effect in the lower cone, 2ndly by showing the smallness of the difference caused by the introduction of athermanous disks), and from comparison with recent results of Magnus, that the effect of radiation in all the cases tried is negligible if not nothing.

By measuring resistance rather than conductivity, several sources of experimental error are eliminated. If the two cones are brought into actual contact, and water of a known temperature is led for a given time through the upper cone, a certain thermal effect is produced in the lower one. If the cones be then separated, and a liquid be interposed between them, and if water of the same temperature as before be led for the same time as before through the upper cone, a less thermal effect is produced. *The difference between the two effects is a measure of the resistance of the liquid.* Results so obtained have to be corrected for the varying pressure to which the air in the lower cone is subjected as the water in the glass tube sinks. To find the absolute results in thermal units, we have to take into account the diameter of the surface of the lower cone, its capacity, and the specific heat of the air which is in it.

The following are the chief results obtained :—

(1) The connexion, in the instance of water, between the thickness and the time required for the first heat-effect.

(2) The connexion between the temperature and the time required for the first heat-effect. It is shown that hotter water conducts heat better than colder, and that the hotter the conducting-water the greater is the difference in rate.

(3) The connexion between the entire quantity of heat passing in a given time and the thickness and temperature of the conducting-water.

(4) The effect of the solution of various salts in altering the thermal resistance of water. Every salt tried was found, when dissolved in water, to increase its thermal resistance. The author submits that the effect of the dissolved salt is chiefly, perhaps wholly, due to the displacement of a portion of the water by a substance having greater resistance, and to the modification in the specific heat of the liquid caused by the introduction of the salt.

(5) The resistance of the liquids in the following list was examined under precisely similar circumstances. The thickness was in each case 1 millim. The initial temperature of the liquid was $20°\cdot17$ C., and the temperature-difference, ΔT, was $10°$C. That is, the platinum surface of the upper cone was maintained at $30°\cdot17$ C. The duration of the experiment in each case was $1'$. The numbers show the specific resistance under the above circumstances—that is, the ratio between the quantities of heat arrested by the several liquids and that arrested by water.

Liquid.	Specific resistance.	Liquid.	Specific resistance.
Water	1·	Acetate of amyl	10·00
Glycerine	3·84	Amylamin	10·14
Acetic acid (glacial)	8·38	Amylic alcohol	10·23
Acetone	8·51	Oil of turpentine	11·75
Oxalate of ethyl	8·85	Nitrate of butyl	11·87
Sperm-oil	8·85	Chloroform	12·10
Alcohol	9·08	Bichloride of carbon	12·92
Acetate of ethyl	9·08	Mercury amyl	12·92
Nitrobenzol	9·86	Bromide of ethylen	13·16
Oxalate of amyl	10·06	Iodide of amyl	13·27
Butylic alcohol	10·00	Iodide of ethyl	(?)

The more salient points of these results are pointed out, such as the preeminently small resistance of water and of bodies containing a large proportion of the elements of water (potential water), the possible connexion of this fact with the results of Magnus concerning the conductivity of hydrogen, the increased resistance accompanying increased molecular complexity in the case of isotypic liquids as exemplified by the alcohols and their derivatives, the great resistance shown by the liquids containing halogens. The results obtained by Tyndall in regard to relative diathermancy are shown to be in accord with the author's results concerning resistance. *A highly diathermanous liquid invariably offers great resistance to conducted heat.* The relation between electrical and thermal resistance in the case of liquids is also briefly discussed.

Jan. 28.—Lieut.-General Sabine, President, in the Chair.

The following communication was read:—
" On Hydrofluoric Acid." By G. Gore, F.R.S.

A. *Anhydrous Hydrofluoric Acid.*

This paper contains a full description of the leading physical and chemical properties of anhydrous hydrofluoric acid, and also an account of various properties of pure aqueous hydrofluoric acid. The author obtained the anhydrous acid by heating dry double fluoride of hydrogen and potassium to redness in a suitable platinum apparatus (shown by a figure accompanying the paper), and states the conditions under which it may be obtained in a state of purity.

The composition and purity of the anhydrous acid are shown and carefully verified by various methods of analysis, both of the double fluoride from which it was prepared and of the acid itself; and particulars are given of all the circumstances necessary to insure reliable and accurate results. Nearly all the operations of preparing, purifying, analyzing, and examining the properties of the acid were conducted in vessels of platinum, with lutings of paraffin, sulphur, and lampblack; articles of transparent and colourless fluor-spar were also employed in certain cases. Nearly all the manipulations with the acid were effected while the vessels containing it were immersed in a strong freezing-mixture of ice and crystallized chloride of calcium.

The pure anhydrous acid is a highly dangerous substance, and

requires the most extreme degree of care in its manipulation. It is a perfectly colourless and transparent liquid at 60° Fahr., very thin and mobile, extremely volatile, and densely fuming in the air at ordinary temperatures, and absorbs water very greedily from the atmosphere. It was perfectly retained in platinum bottles, the bottle having a flanged mouth with a platinum plate secured with clamp-screws, and a washer of paraffin.

A number of attempts were made, finally with success, to determine the molecular volume of the pure anhydrous acid in the gaseous state, the acid in these cases being prepared by heating pure anhydrous fluoride of silver with hydrogen in a suitable platinum apparatus over mercury. Particulars are given of the apparatus employed and of the manipulation. The results obtained show that one volume of hydrogen, in uniting with fluorine, produces not simply one volume of gaseous product as it does when uniting with oxygen, but two volumes, as in the case of its union with chlorine. The gaseous acid transferred to glass vessels over mercury did not corrode the glass, or render it dim in the slightest degree during several weeks, provided that moisture was entirely absent.

The author concludes that the anhydrous acid he has obtained is destitute of oxygen, not only from the various analyses and experiments already referred to, but also, 1st, because the double fluoride from which it was prepared, when fused and electrolyzed with platinum electrodes, evolved abundance of inflammable gas at the cathode, but no gas at the anode, although oxides are by electrolysis decomposed before fluorides; 2nd, because the electrolysis of the acid with platinum electrodes yielded no odour of ozone, whereas the aqueous acid of various degrees of strength evolved that odour strongly; and, 3rd, because the properties of the acid obtained from hydrogen and fluoride of silver agree with those of the acid obtained from the double salt. He considers also that the acid obtained from pure fluor-spar and monohydrated sulphuric acid heated together in a platinum retort is free from oxygen and water.

The specific gravity of the anhydrous liquid acid was several times determined, both in a specific-gravity bottle of platinum, and also by means of a platinum float submerged and weighed in the acid. Concordant and reliable results were obtained; the specific gravity found was $0 \cdot 9879$ at $55°$ Fahr., that of distilled water being $= 1 \cdot 000$ at the same temperature.

The anhydrous acid was much more volatile than sulphuric ether. Its boiling-point was carefully determined in a special apparatus of platinum, and was found to be $67°$ Fahr. Not the slightest sign of freezing occurred on cooling the acid to $-30°$ Fahr. $(=-34° \cdot 5$ C.$)$; and it is highly probable that its solidifying temperature is a very great many degrees below this. Its vapour-tension at $60°$ Fahr. was also approximately determined, and was found to be $=7 \cdot 58$ lbs. per square inch. On loosening the lid of a bottle of the acid at $60°$ Fahr., the acid vapour is expelled in a jet like steam from a boiler; this, together with the low boiling-point, the extremely dangerous and corrosive nature of the acid, and its great affinity for

water, illustrates the very great difficulty of manipulating with it and retaining it in a pure state. Nevertheless, by the contrivances described, and by placing the bottles in a cool cellar (never above a temperature of 60° Fahr.), the author has succeeded in keeping the liquid acid perfectly, without loss and unaltered, through the whole of the recent hot summer.

The electrical relations of different metals &c. in the acid were found to be as follows at 0° Fahr. :—zinc, tin, lead, cadmium, indium, magnesium, cobalt, aluminium, iron, nickel, bismuth, thallium, copper, iridium, silver, gas-carbon, gold, platinum, palladium.

Numerous experiments were made of electrolyzing the anhydrous acid with anodes of gas-carbon, carbon of lignum-vitæ and of many other kinds of wood, of palladium, platinum, and gold. The gas-carbon disintegrated rapidly ; all the kinds of charcoal flew to pieces quickly ; and the anodes of palladium, platinum, and gold were corroded without evolution of gas. The acid with a platinum anode conducted electricity much more readily than pure water ; but with one of gold it scarcely conducted at all. These electrolytic experiments presented extreme difficulties, and were conducted in a platinum apparatus (shown by a figure) specially devised for the purpose. The particulars of the conditions and results obtained are described in the paper. Various mixtures of the anhydrous acid with mono-hydrated nitric acid, with sulphuric anhydride, and with monohydrated sulphuric acid were also electrolyzed by means of platinum anodes, the particulars and results of which are also described.

To obtain an idea of the *general* chemical behaviour of the pure anhydrous acid, numerous substances (generally anhydrous) were immersed in separate portions of the acid in platinum cups, kept at a low temperature (0° to —20° Fahr.). The acid had scarcely any effect upon any of the metalloids or noble metals ; and even the base metals in a state of fine powder did not cause any evolution of hydrogen. Sodium and potassium behaved much the same as water. Nearly all the salts of the alkali and alkaline-earth metals produced strong chemical action. Various anhydrides (specified) dissolved freely. Strong aqueous hydrochloric acid produced active effervescence. The alkalies and alkaline earths united strongly with the acid. Peroxides gave no effect. Numerous oxides (specified) produced strong chemical action, some of them dissolving. Some nitrates were not chemically affected ; others (those of lead, barium, and potassium) were decomposed. Fluorides generally were unchanged ; but those of the alkali metals and of thallium produced different degrees of chemical action, those of ammonium, rubidium, and potassium uniting powerfully. Numerous chlorides were also unaffected, whilst those of phosphorus (the *solid* one only), antimony (the perchloride), titanium, and of the alkaline-earth and alkali metals, were decomposed with strong action, and generally with effervescence. The chlorates of potassium and sodium were also decomposed with evolution of chloric acid ; the bromides of the alkaline-earth and alkali metals behaved like their chlorides. Bromate of potassium rapidly set free bromine. Numerous iodides were unaf-

fected; but those of the alkaline-earth and alkali metals were strongly decomposed, and iodine (in some cases only) set free. The anhydrous acid decomposed all carbonates with effervescence, and those of the alkaline-earth and alkali metals with violent action. Borates of the alkalies also produced very strong action. Silico-fluorides of the alkali metals dissolved with effervescence. All sulphides, except those of the alkaline-earth and alkali metals, exhibited no change; the latter evolved sulphuretted hydrogen violently. Bisulphite of sodium dissolved with effervescence. Sulphates were variously affected. The acid chromates of the alkali metals dissolved with violent action to blood-red liquids, with evolution of vapour of fluoride of chromium. Cyanide of potassium was violently decomposed, and hydrocyanic acid set free. Numerous organic bodies (specified) were also immersed in the acid; most of the solid ones were quickly disintegrated. The acid mixed with pyroxylic spirit, ether, and alcohol, but not with benzole; with spirit of turpentine it exploded, and produced a blood-red liquid. Gutta percha, india-rubber, and nearly all the gums and resins were rapidly disintegrated and generally dissolved to red liquids. Spermaceti, stearic acid, and myrtle wax were but little affected, and paraffin not at all. Sponge was also but little changed. Gun-cotton, silk, paper, cotton-wool, calico, gelatine, and parchment were instantly converted into glutinous substances, and generally dissolved. The solution of gun-cotton yielded an inflammable film on evaporation to dryness. Pinewood instantly blackened.

From the various physical and chemical properties of the anhydrous acid, the author concludes that it lies between hydrochloric acid and water, but is much more closely allied to the former than to the latter. It is more readily liquefied than hydrochloric acid, but less readily than steam; like hydrochloric acid it decomposes all carbonates; like water it unites powerfully with sulphuric and phosphoric anhydrides, with great evolution of heat. The fluorides of the alkali metals unite violently with hydrofluoric acid, as the oxides of those metals unite with water; the hydrated fluorides of the alkali metals also, like the hydrated fixed alkalies, have a strongly alkaline reaction, and are capable of expelling ammonia from its salts. It may be further remarked that the atomic number of fluorine lies between that of oxygen and chlorine; and the atomic number of oxygen, added to that of fluorine, nearly equals that of chlorine.

B. *Aqueous Hydrofluoric Acid.*

Under the head of the aqueous acid the author enumerates the various impurities usually contained in the commercial acid, and describes the modes he employed to detect and estimate them, and to estimate the amount of H F in it. The process employed by him for obtaining the aqueous acid in a very high degree of purity from the commercial liquid, is also fully described. It consists essentially in passing an excess of sulphuretted hydrogen through the acid, then neutralizing the sulphuric and hydrofluosilicic acids present by carbonate of potassium, decanting the liquid after subsidence

of the precipitate, removing the excess of sulphuretted hydrogen by carbonate of silver, distilling the filtered liquid in a leaden retort with a condensing-tube of platinum, and, finally, rectifying.

The effect of cold upon the aqueous acid was briefly examined, the result being that a comparatively small amount of hydrofluoric acid lowers the freezing-point of water very considerably.

The chemico-electric series of metals &c. in acid of 10 per cent. and in that of 30 per cent. were determined. In the latter case it was as follows:—zinc, magnesium, aluminium, thallium, indium, cadmium, tin, lead, silicon, iron, nickel, cobalt, antimony, bismuth, mercury, silver, copper, arsenic, osmium, ruthenium, gas-carbon, platinum, rhodium, palladium, tellurium, osmi-iridium, gold, iridium. Magnesium was remarkably unacted upon in the aqueous acid. The chemico-electric relation of the aqueous acid to other acids with platinum was also determined.

Various experiments of electrolysis of the aqueous acid of various degrees of strength were made with anodes of platinum. Ozone was evolved, and, with the stronger acid only, the anode was corroded at the same time. Mixtures of the aqueous acid with nitric, hydrochloric, sulphuric, selenious, and phosphoric acids were also electrolyzed with a platinum anode, and the results are described.

LXVI. *Intelligence and Miscellaneous Articles.*

ON THE VOLTAIC DEPORTMENT OF PALLADIUM.
BY J. C. POGGENDORFF.

IN his remarkable research on hydrogenium, Mr. Graham has shown, among other things, that palladium when it takes up hydrogen expands, and, when the hydrogen escapes, contracts apparently more strongly than it had previously expanded. A palladium wire which originally measured 609·144 millims., and by absorbing hydrogen had become longer by 9·77 millims., after the hydrogen had been expelled, was found to measure 599·444 millims., and had therefore shortened to the extent of 9·7 millims.

Where no exact numerical determinations are required, both phenomena may be very strikingly demonstrated if palladium is charged with hydrogen by electrolysis, a very thin plate being used. I used a plate which was 118 millims. long by 23 millims. broad, and only 1 millim. thick, placed at a distance of 8 millims. from a platinum plate in dilute sulphuric acid.

If this be connected with a small Grove's battery of two elements, so that the palladium becomes charged with hydrogen, the following is observed.

Even after a few minutes the palladium plate begins to bend away from the platinum, and gradually becomes greatly curved. After about a quarter of an hour this has reached its maximum. A curvature then commences in the opposite direction, so that the plate first becomes straight, and then curves towards the platinum, which

in a short time goes so far that the plates come into contact, by which the electrolytic process, of course, is stopped.

These curvatures are obviously due to the fact that the sides of the palladium plate are successively saturated with hydrogen, and therefore expand, just as does a Breguet's spiral by an alteration in temperature.

Just as the expansion of palladium by absorbing hydrogen may be rendered obvious by this experiment, so the contraction of the metal by the escape of the gas may be shown even more strikingly.

For this purpose the palladium plate, after having attained the maximum of its first curvature, is removed from the liquid, washed, dried, and brought into a spirit-lamp flame. As soon as it is suffi- ciently hot, it curves in the opposite direction with extreme rapi- dity, and so much that it seems to be regularly rolled up.

In these contractions and expansions of the plate its other dimen- sions also undergo change. When the process of charging a plate with hydrogen, and afterwards expelling it by heat, has been fre- quently repeated on the same plate, it can be distinctly seen that it has not only become shorter, but also narrower and thicker. After the process had been repeated six times my plate was 8 mil- lims. shorter and 1·5 millim. narrower, but at the same time was quite 0·1 millim. thicker. That dimension, therefore, which is com- pressed by rolling expands, and the two others, by which the metal is stretched, crumple up when the hydrogen is expelled. Graham has already shown, by the decrease of its specific gravity, that a palladium wire is thicker when it becomes shorter.

It may, in conclusion, be remarked that, although Graham and Wurtz have not succeeded in preparing a hydride of palladium by purely chemical processes, such a compound appears to be formed by the electrolytic process; for the dilute sulphuric acid in which this process is performed becomes of a brown colour, without be- coming turbid or depositing anything. A solution of caustic pot- ash or ammonia in which, according to an old observation of mine, tellurium used as negative pole produces a beautiful and deep wine co- loration, remains quite clear with palladium.—Poggendorff's *Annalen*, March 1869.

ELECTRICAL POLARITY AND INEQUALITY OF THE AMALGAMATED ZINC ELECTRODES IN SULPHATE OF ZINC. BY E. PATRY.

In a series of experiments on the galvanic resistance of liquids, in which I assisted Dr. Paalzow, polarization and galvanic ine- quality of the so-called unpolarizable electrodes were met with as sources of error. I was thereby led to examine this subject speci- ally, in which I had the advantage of his counsel.

Unpolarizable electrodes are obtained by using amalgamated zinc and sulphate of zinc. When I used zinc and commercial zinc vitriol, I found that the polarization and inequality were very di- stinct as soon as the resistance of the circuit was not very great. I observed that a number of gas-bubbles were liberated at the surface

of the electrodes ; and a flame kindled these bubbles, which showed that the gas in them was hydrogen. Assuming that free sulphuric acid was present in the solution, I saturated it with carbonate of zinc by adding this to the solution, which I kept boiling for two or three hours. This is necessary ; for experiment showed me that a simple addition has but a slight action. The filtered solution contained sulphate of zinc, an extremely small quantity of carbonate of zinc, and basic sulphate of zinc, which had been formed during the boiling. This mixture gave a far smaller polarization.

I then compared the polarization and the inequality which occur in ordinary sulphate of zinc and those which are exhibited by the solution I had treated with carbonate of zinc, and measured these various values.

I took for this purpose glass troughs 80 millims. in length, 30 millims. in breadth and in height. The zinc electrodes measured 75 by 18 millims. By means of a tongue on the zinc plate the conducting-wire could be fastened outside the liquid.

By means of a commutator this trough could be inserted in a circuit consisting of a mirror-galvanometer and of a Wheatstone's bridge. The same commutator placed the electrodes in connexion when the apparatus was outside the circuit.

The resistance of the galvanometer was 694 Siemens's units, that of the trough only 3 to 4. The latter could be neglected as compared with the former.

On the other hand, the Daniell's element which fed the bridge was provided with a rheostat of 162 units, by which the variations in the battery (the resistance of which amounted to about 1 unit) could be neglected. This rheostat and the position of the slider were so arranged that the battery could only yield a hundredth of its electromotive force.

Some preliminary experiments showed me that the battery was sufficiently constant.

First experiment. *Crude sulphate of zinc.*—

The *inequality* gave the following numbers for the deflections of the mirror :—

millims.		minutes.
18	after	1
20	,,	5
23	,,	10
15	,,	30
12	,,	20
12	,,	40
12	,,	60

The number 12 may be regarded as constant after the lapse of a certain time. We shall then further see how the electromotive force may be deduced from this number.

Polarization.—The current passed first through the trough, and then directly gave, first, the force of the current minus the polarization and, according to the direction, plus or minus the inequality ; secondly, the current alone.

Let C be the current, P the polarization, and I the inequality; the current gave the following results:—

$$\left.\begin{array}{r}\text{millims.}\\ \mathrm{C-P-I}=498\\ \mathrm{C}=519\\ \mathrm{P+I}=\ \ 21\end{array}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

therefore

The current sent in the other direction,

$$\left.\begin{array}{r}\mathrm{C-P+I}=487\\ \mathrm{C}=484\\ \mathrm{I-P}=\ \ \ 3\end{array}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Combining (1) and (2) we obtain

millims.
Inequality............ 12
Polarization 9

These experiments thus confirm the result found above for the inequality.

Second experiment. *Sulphate of zinc treated with carbonate of zinc.* —I gives very quickly

$$\mathrm{I}=3 \text{ millims.} = \text{constant};$$

upon this the current passed

$$\mathrm{C-P+I}=462,$$
$$\mathrm{C}=459.$$

In the opposite direction,

$$\mathrm{C-P-I}=466,$$
$$\mathrm{C}=469,$$

from which

$$\mathrm{I}=3, \quad \mathrm{P}=0.$$

Calculation of the electromotive force resulting from the inequality, and of that from the polarization of the electrodes.

We have a circuit in which three electromotive forces may be introduced. They are proportional to the deflection produced by each of them. If 500 millims. is the mean deflection produced by the hundredth of the battery, the *electromotive force* corresponding to *one* millimetre will be 0·00002.

We conclude from this that the electromotive force arising from the inequality is, in the case of the first solution,

0·00024 of a Daniell's cell,

and with the second solution

0·00006.

The electromotive force resulting from the polarization is in the first case 0·00018,

and in the second case null.

The same method, applied to sulphuric acid of 1·08 at 27° C. and to amalgamated zinc electrodes, gives

$$\mathrm{I}=25 \text{ millims. and } \mathrm{C}=15 \text{ millims.}$$

Since the battery produces a deflection of 480 millims., the electromotive forces are

$$I = \frac{25}{48000} = 0\cdot00052,$$

$$P = \frac{15}{48000} = 0\cdot000312.$$

We thus see that, using amalgamated zinc electrodes, *sulphate of zinc* gives considerable *inequality* and *polarization* if it contains free sulphuric acid and the resistances are small, but that the former is reduced to a quarter and the latter to zero if the free acid is neutralized with carbonate of zinc.—*Arch. des Scienc. Phys. et Nat.* Nov. 1868.

ON A DEVELOPMENT OF HEAT WHICH ACCOMPANIES THE BURSTING OF THE PRINCE RUPERT'S DROPS. BY M. DUFOUR.

The bursting of a Prince Rupert's drop is accompanied by a violent projection of the substance of the glass. A molecular repulsion seems to be produced in the interior of the body, which is strong enough to impart to the separating particles a great velocity. At the moment of explosion there is a considerable development of *vis viva*, quite out of proportion to the feeble mechanical effort consumed in bursting the point. From the latter point of view, the Rupert's drops suggest a problem concerning the mechanical theory of heat ; and it is natural to inquire whether the work which accompanies this explosion is not accompanied by calorific phenomena.

At first sight it would seem as if there were some analogy between the separation of the vitreous particles in a bursting drop and the sudden dilatation of a gas which expands after compression. The small amount of work which consists in opening a stopcock renders possible a considerable amount of work in the expanding gas, just as the small effort necessary to break the point of a tear gives rise to the reduction to powder and the projection of a large mass of glass. Yet between these facts, the analogy of which I point out, there are so many differences of an essential character, that we cannot predict whether the explosion of the Rupert's drops is accompanied by thermal phenomena like those which accompany the expansion of a gas.

This thermomechanical problem, which is difficult to discuss *à priori*, appeared important enough to merit some investigation ; and I have endeavoured to ascertain whether the explosion is accompanied by a change in temperature of the substance of the glass.

In a first series of experiments a thermo-electric apparatus was used, to ascertain whether there was any difference of temperature between the drops and the powder they produce. All the results agreed, and showed a heating of the substance of the glass ; the method employed did not enable me to measure with accuracy the increase of temperature, which, moreover, was very slight.

In a second series of experiments the drops were broken in a small brass vessel containing oil of turpentine, which received the powder

after explosion. The usual precautions were taken to avoid the influence of the surrounding temperature, and to measure the quantity of heat produced in the vessel *. The observations, properly calculated, have shown that the powder had always a temperature higher by $0°·18$ to $0°·45$ than that of the drops before the explosion. By this method it was impossible to avoid a very violent agitation of the liquid, produced by the contact of the bursting tears ; the results thus obtained are therefore liable to important errors.

In a third series of experiments the drops were placed in a sort of truncated cone of cardboard, the axis of which was almost vertical ; the base of the cone was uppermost, and was closed by a sheet of caoutchouc. The points just projected through this sheet far enough to be seized with a pair of nippers and broken. The vitreous powder fell into a very thin cylinder of brass, suitably placed below the cone, and containing some grammes of oil of turpentine. The temperature of the drops before bursting was observed, as well as that of the turpentine before and after the fall of the powdered glass. The apparatus was protected, of course, against the effects of the surrounding temperature ; and the observations were calculated so as to furnish the difference between the temperature of the fragments of glass and that of the drops which produced them. All the results indicate an increase in the temperature of the glass on its bursting ; the results, which are in better agreement than those of the preceding series, are comprised between $0°·26$ and $0°·35$. Control experiments were made with the view of proving that the increase of temperature could not be due either to the mere fall of the glass into the oil, or to some capillary action between the liquid and the powder of the solid.

All the experiments made authorize the following conclusions :—

(1) At the time of the explosion of the drops the glass powder which is produced has a higher temperature than that of the drops themselves at the time of the explosion.

(2) A series of six experiments (third method) made with eighteen drops having a mean weight of $4·8$ grms. indicated a mean excess of $0°·30$.

This heating probably derives its origin from the molecular motion which accompanies an explosion ; and I think it can be brought into connexion with the phenomena produced when metallic wires, having been elongated (without exceeding the limit of elasticity), suddenly resume their original length. Mr. Joule has shown † that wires become cooler when drawn out, and then heat again when they resume their original volume. In a recent memoir ‡ M. Edlund has published similar results, obtained by drawing out wires of different metals, and then allowing them to cool.

* The details of these experiments will be found in a memoir in an early Number of the *Archives des Sciences Physiques et Naturelles de Genève.*

† Phil. Trans. for 1858. Phil. Mag. vol. xv. p. 538.

‡ *Ann. de Chim. et de Phys.* vol. lxiv., and Pogg. *Ann.* 1865. Sir W. Thomson has given a formula, deduced from the mechanical theory of heat, by which these variations of temperature may be calculated.

In the preparation of a Rupert's drop the sudden cooling solidifies the external coating of the glass while the interior is still liquid. This layer surrounds, therefore, a volume greater than the volume of the cooled glass will occupy. During cooling, the internal mass is connected by adherence to the outside layer already formed. The glass cannot, therefore, undergo its normal contraction ; it undergoes a traction which tends to keep its volume larger, and its condition must greatly resemble that of a metallic bar elongated by an external effort. A Rupert's drop may be regarded as a rigid envelope, inside which bars raised to a high temperature have been fixed to the sides, these bars being in considerable numbers, interlacing one another, and connected by countless solderings. During cooling, all these bars in contracting would undergo a traction on the part of the rigid envelope, they would be drawn out ; and the whole system would be in a condition of unstable equilibrium. In a Rupert's drop the infinitely small and infinitely numerous particles of the glass play the part of the bars in question, and they contract at the moment of breaking. The contraction of these particles, like that of the metal wires in the experiments of Messrs. Joule and Edlund, is accompanied by very small molecular displacements, to which a state of repose quickly succeeds. *Vis viva* disappears, and it is to be expected that at the same time a certain quantity of heat will appear. This is doubtless the origin of the heat observed in the experiments above described.

But the return to the condition of stable equilibrium in a Rupert's drop is accompanied by this remarkable and sudden projection of the particles of glass, which gives the phenomenon the appearance of an explosion. This is a very curious phenomenon, and one difficult to explain in a satisfactory manner. It may perhaps arise from particles of glass only attaining their volume and their form of stable equilibrium after some oscillations resembling those which a spring performs when suddenly let go. When an elastic body is in vibration, it impels with more or less velocity foreign bodies in contact with it. If the spring itself is moveable and it strikes against fixed bodies, the reaction will send it in the opposite direction. If a considerable number of small elastic fragments undergo vibrations and are, moreover, in contact, they strike against and mutually repel each other. In a Rupert's drop it may be supposed that at the time of rupture the particles of glass, till then drawn out, vibrate also for a short time before attaining their position of stable equilibrium. The mutual shocks which are thus produced are perhaps the cause of the remarkable projection suffered by the débris of an exploded drop.

Moreover, whatever may be the immediate cause of this projection, it is probable that the motion thus produced in the glass consumes a portion of the internal heat, a fraction of which is partially regenerated in the fragments when they are stopped by external resistances.— *Comptes Rendus*, February 15, 1869.

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